Policing Cannabis and Drug Related Hospital Admissions: Evidence from Administrative Records*

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Abstract

We evaluate the impact of a policing experiment that depenalized the possession of small quantities of cannabis in the London borough of Lambeth, on hospital admissions related to illicit drug use. To do so, we exploit administrative records on individual hospital admissions classified by ICD-10 diagnosis codes. These records allow the construction of a quarterly panel data set by London borough running from 1997 to 2009 to estimate the short and long run impacts of the depenalization policy unilaterally introduced in Lambeth between 2001 and 2002. We find the depenalization of cannabis had significant longer term impacts on hospital admissions related to the use of hard drugs, raising raising hospital admission rates for men by between 40 and 100% of their pre-policy baseline levels. Among Lambeth residents, the impacts are concentrated among men in younger age cohorts, and among those with no prior history of hospitalization related to illicit drug or alcohol use. The dynamic impacts across cohorts vary in profile with some cohorts experiencing hospitalization rates remaining above pre-intervention levels six years after the depenalization policy is introduced. We find evidence of smaller but significant positive spillover effects in hospitalization rates related to hard drug use among residents in boroughs neighboring Lambeth, and these are again concentrated among younger cohorts without prior histories of hospitalizations related to illicit drug or alcohol use. We combine these estimated impacts on hospitalization rates with estimates on how the policy impacted the severity of hospital admissions to provide a lower bound estimate of the public health cost of the depenalization policy.

Keywords: cannabis, Class-A drugs, depenalization, hospital admissions.
JEL Classification: I18, K42, H75.

1   Introduction

Illicit drug use generates substantial economic costs including those related to crime, ill-health, and diminished labor productivity. In 2002, the Office for National Drug Control Policy estimated that illicit drugs cost the US economy $181 billion in total [Office for National Drug Control Strategy, 2004]; for the UK, Gordon *et al.* [2006] estimated the total cost of drug-related crime and health service use to be £15.4 billion in 2003/4. It is these social costs, coupled with the risks posed to drug users themselves, that have led governments throughout the world to try and regulate illicit drug markets. All such policies aim to curb both drug use and its negative consequences, but there is ongoing debate amongst policy-makers as to relative weight that should be given to policies related to prevention, enforcement, and treatment [Grossman *et al.*, 2002].

The current trend in policy circles is to suggest regimes built *solely* around strong enforcement and punitive punishment might be both costly and ineffective. For example, after forty-years of the US 'war on drugs', the Obama administration has adopted a strategy that focuses more on prevention and treatment, and less on incarceration [Office for National Drug Control Strategy, 2011], although other federal agencies such as the Drug Enforcement Agency and the Office for National Drug Control Policy remain more focused on traditional incarceration-based approaches. Other countries such as the Netherlands, Australia and Portugal, have long adopted more liberal approaches that have depenalized or decriminalized the possession of some illicit drugs, most commonly cannabis, with many countries in Latin America currently debating similar moves. While such policies might help free up resources from the criminal justice system, these more liberalized policies also carry their own risks: if such policies signal the health and legal risks from consumption have been reduced, then this should reduce prices [Becker and Murphy, 1988], potentially increasing the number of users as well as increasing use among existing users, all of which could have deleterious consequences for user's health. The use of certain drugs might also provide a causal 'gateway' to more harmful and addictive substances [van Ours, 2003; Melberg *et al.*, 2010], and possible impacts onto other forms of anti-social behavior beyond criminal activity.

This paper considers the impact of a localized policing experiment that reduced the enforcement of punishments against the use of one illicit drug - cannabis - on a major cost associated with the consumption of illegal drugs: the use of health services by consumers of illicit drugs. The policing experiment we study took place unilaterally in the London Borough of Lambeth and ran from July 2001 to July 2002, during which time all other London boroughs had no change in policing policy towards cannabis or any other illicit drug. The experiment - known as the Lambeth Cannabis Warning Scheme (LCWS) - meant that the possession of small quantities of cannabis was temporarily depenalized, so that this was no longer a prosecutable offence.[1] We evaluate the

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[1]Donohue *et al.* [2011] categorize illicit drug policies into three type: (1) legalization - a system in which possession and sale are lawful but subject to regulation and taxation; (ii) criminalization - a system of proscriptions on possession and sale backed by criminal punishment, potentially including incarceration; (iii) depenalization - a hybrid system, in which sale and possession are proscribed, but the prohibition on possession is backed only by such

short and long run consequences of this policy on healthcare usage as measured by detailed and comprehensive administrative records on drug-related admissions to all London hospitals. Such hospital admissions represent 60% of drug-related healthcare costs [Gordon *et al.*, 2006]. To do so we use a difference-in-difference research design that compares pre and post-policy changes in hospitalization rates between Lambeth and other London boroughs. Our analysis aims to shed light on the broad question of whether policing strategies towards the market for cannabis impact upon public health, through changes in the use of illicit drugs and subsequent health of drug users.

Our primary data comes from a novel source that has not been much used by economists: the Inpatient Hospital Episode Statistics (HES). These administrative records document every admission to a public hospital in England, with detailed ICD-10 codes for classifying the primary and secondary cause of each individual hospital admission.[2] This is the most comprehensive health related data available for England, in which it is possible to track the admissions history of the same individual over time. We aggregate the individual HES records to construct a panel data set of hospital admissions rates by London borough and quarter. We do so for various cohorts defined along the lines of gender, age at the time of the implementation of the depenalization policy, and previous hospital admissions history. As such these administrative records allow us to provide detailed evidence on the aggregate impact of the depenalization policy on hospitalization rates, and to provide novel evidence on how these health impacts vary across population cohorts.

The balanced panel data we construct covers all 32 London boroughs between April 1997 and December 2009. This data series starts four years before the initiation of the depenalization policy in the borough of Lambeth, allowing us to estimate policy impacts accounting for underlying trends in hospital admissions. The series runs to seven years after the policy ended, allowing us to assess the long term impacts of a short-lived change in policing strategy related to cannabis.

Given the detailed ICD-10 codes available for each admission, the administrative records allow us to specifically measure admission rates for drug-related hospitalizations for *each* type of illicit drug: although the depenalization policy would most likely impact cannabis consumption more directly than the usage of other illicit drugs [Chu, 2012], this has to be weighed against the

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sanctions as fines or mandatory substance abuse treatment, not incarceration. The LCWS policing experiment we evaluate is a policy of depenalization. The practical way in which it was implemented is very much in line with policy changes in other countries that have changed enforcement strategies in illicit drug markets and as such we expect our results to have external validity to those settings, including for the current debate on the potential decriminalization of cannabis in California [Kilmer *et al.*, 2010]. As discussed in [Chu, 2012], medical marijuana legislation represents a major change in US policy in recent years, where 17 states have now passed laws that allow individuals with specific symptoms to use marijuana for medical purposes.

[2]Private healthcare constitutes less than 10% of the healthcare market in England, with most admissions for elective procedures. Focusing on admissions to public hospitals is therefore unlikely to produce a biased evaluation of the policing policy on drug-related hospitalizations. The HES contains an inpatient and an outpatient data set. We only use the inpatient data. The inpatient data includes all those admitted to hospital (under the order of a doctor) who are expected to stay at least one night, and contains ICD-10 diagnosis classifications. The outpatient data covers those in which a patient is seen but does not require a hospital bed for recovery purposes (except for a short recovery after a specific procedure). We do not use the HES outpatients data because it is only reliable from 2006/7 onwards (and so not before the LCWS is initiated) and does not have information on diagnosis codes.

fact that hospitalizations related to cannabis usage are extremely rare and so policy impacts are statistically difficult to measure along this margin. Our main outcome variable therefore focuses on hospital admissions related to hard drugs, known as 'Class-A' drugs in England. This includes all hospital admissions where the principal diagnosis relates to cocaine, crack, crystal-meth, heroin, LSD, MDMA or methadone.[3] The administrative records also contain information on the length of hospital stays (in days) associated with each patient admission, and we use this to explore whether the depenalization policy impacted the *severity* of hospital admissions (not just their incidence), where the primary diagnosis relates to hospitalizations for Class-A drug use. Ultimately, we then combine the estimated policy impacts on hospitalization rates, with the estimated policy impacts on the severity of hospital admissions for Class-A drug use, to provide a conservative estimate of the public health costs of the depenalization policy that arises solely through the increased demand on hospital bed services.

We present four main results. First, relative to other London boroughs, the depenalization policy had significant long term impacts on hospital admissions in Lambeth related to the use of Class-A drugs, with the impacts being concentrated among men. Exploring the heterogeneous impacts across male cohorts, we find the direct impacts on Lambeth residents to be larger among cohorts that were younger at the start of the policy, and concentrated especially among those with no prior history of hospitalization related to drug or alcohol use. The magnitudes of the impacts are large across age cohorts: the increases in hospitalization rates correspond to rises of between 40 and 100% of their pre-policy baseline levels in Lambeth, for those aged 15-24 and aged 25-34 on the eve of the policy. As far as our data allows, not much of the impact appears to arise from drug users changing borough of residence over time with net inflows into Lambeth.

Second, the dynamic impacts across cohorts vary in profile with some cohorts experiencing hospitalization rates remaining above pre-intervention levels six years after the depenalization of cannabis was first introduced. Third, we find evidence of positive spillover effects on hospitalizations related to Class-A drug use among those resident in boroughs neighboring Lambeth. These spillovers are again concentrated among younger cohorts without prior histories of hospital admissions related to the use of illicit drugs or alcohol. As expected, the magnitude of these spillover effects are significantly smaller than the direct impacts documented among Lambeth residents. Finally, the severity of hospital admissions, as measured by the length of stay in hospital, significantly increases for admissions related to Class-A drug use. Taking the main within-Lambeth channels through which the policy impacts public health through increased bed-days in hospital, we estimate the annual cost of the policy to more than offset the downward time trend in hospital

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[3]The UK has a three tiered drug classification system, with assignment from Class-C to Class-A intended to indicate increasing potential harm to users. Class-A drugs include cocaine, crack, crystal-meth, heroin, LSD, MDMA and methadone. Much of the ongoing policy debate on the decriminalization or depenalization of cannabis, reclassifying it from Class-B to Class-C, stems from the fact that legal drugs such as alcohol and tobacco, are thought to have higher levels of dependency and cause more physical harm to users than some illicit drugs including cannabis [Nutt *et al.*, 2007].

bed-day costs that is found to exist on average across all other London boroughs in the post-policy period.

Taken together, our results suggest policing strategies towards the market for cannabis have significant, nuanced and long lasting impacts on public health.

Our analysis contributes to understanding the relationship between drug policies and public health, an area that has received relatively little attention despite the sizable social costs involved. This partly relates to well known difficulties in evaluating policies in illicit drug markets: multiple policies are often simultaneously targeted towards high supply locations; even when unilateral policy experiments or changes occur they often fail to cause abrupt or quantitatively large demand or supply shocks, and data is rarely detailed enough to pin down interventions in specific drug markets on other drug-related outcomes [DiNardo, 1993; Caulkins, 2000; Chu, 2012]. Our analysis, that combines a focused policy and administrative records, makes some progress on these fronts.

To place our analysis into a wider context, it is useful to compare our findings with two earlier prominent studies of the links between illicit drug enforcement policies and health outcomes: Model [1993] uses data from the mid-1970s to estimate the impact on hospital emergency room admissions of cannabis decriminalization, across 12 US states. She finds that policy changes led to an increase in cannabis-related admissions and a decrease in the number of mentions of other drug related emergency room admissions, suggesting a net substitution towards cannabis. Our administrative records also allow us to also check for such broad patterns of substitution or complementarity between illicit drugs. Our results suggest that the depenalization of cannabis led to longer term increases in the use of Class-A drugs, as measured by hospital inpatient admissions rather than emergency room admissions as in Model [1993].[4]

More recent evidence comes from Dobkin and Nicosia [2009], who assess the impact of an intervention that disrupted the supply of methamphetamine in the US by targeting precursors to methamphetamine. They document how this led to a sharp price increase and decline in quality for methamphetamine. Hospital admissions mentioning methamphetamine fell by 50% during the intervention, whilst admissions into drug treatment fell by 35%. Dobkin and Nicosia [2009] find no evidence that users substituted away from methamphetamine towards other drugs. Finally, Dobkin and Nicosia [2009] find the policy of disrupting methamphetamine supply was effective only for a relatively short period: the price of methamphetamine returned to its pre-intervention level within four months and within 18 months hospital admissions rates had returned to their baseline levels. In contrast, the cannabis depenalization policy we document has an impact on hospitalization rates that, for many cohorts, lasts for up to six years after the policy was initiated

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[4]An important distinction between our data and that used in Model [1993] is that the HES data has a patient-episode as its unit of observation, rather than 'drug mentions' of which Model [1993] report up to six per patient-episode. Moreover, the data used in Model [1993] are not administrative records, but were collected by the Drug Abuse Warning Network from emergency rooms in 24 major SMSAs. As Model [1993] discusses, some data inconsistencies arise because the emergency rooms in the sample change over time.

and despite the fact that the policy itself was only formally in place for one year.[5]

The paper is organized as follows. Section 2 describes the LCWS and the existing evidence on its impact on crime. Section 3 details our administrative data, discusses the plausibility of a link between policing-induced changes in the cannabis market and the consumption of Class-A drugs, and describes our empirical method. Section 4 presents our baseline results which estimate the impact of the LCWS by cohort and the associated robustness checks, and presents some tentative evidence on changes in borough of residence of drug users. Section 5 presents extended results related to dynamic effects, geographic spillovers, the severity of admissions, and the estimated public health costs of the policy. Section 6 discusses the broader implications of our findings for drug policy, and the potential for opening up a research agenda exploring the relationship between police behavior and public health.

2   The Lambeth Cannabis Warning Scheme (LCWS)

The Lambeth Cannabis Warning Scheme (LCWS) was unilaterally introduced into the London borough of Lambeth on 4th July 2001 by the borough's police force. The scheme was initially launched as a pilot intended to last six months, and represented a change in policing policy towards the market for cannabis. Under the scheme, those found in possession of small quantities of cannabis for their personal use in Lambeth: (i) had their drugs confiscated; (ii) were given a warning rather than being arrested. The main reason behind the policy change was to reduce the number of individuals being criminalized for consuming cannabis, and to free up police time and resources to deal with more serious crime, including those related to hard drugs or 'Class-A drugs' [Dark and Fuller, 2002; Adda *et al.*, 2011]. The underlying motivation for the policy, as well as the way in which it was implemented and the targeted outcomes, were very similar to the way in cannabis depenalization policies have often been implemented throughout the world. In keeping with other experiences of depenalization, the primary motivation behind the policy was to free up police time and resources to tackle other crimes, and there was little or no discussion of the depenalization policy's potential impact on public health. To this extent our results can be informative of the existence of links between police drugs policy and public health in settings outside of the specific London context we study.[6]

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[5]As with the economics literature the bulk of the criminology literature has also focused on the *crime* impacts of drug enforcement policies. One exception is Hughes and Stevens [2010] who study the wider impacts of the decriminalization of cannabis introduced in Portugal in 2001. However the evidence they present is based either on Europe wide survey data and compares trends in Portugal to those in Spain and Italy, or stakeholder interviews in Portugal. They do not present regression estimates to measure causal impacts. MacCoun and Reuter [2001] discuss the health impacts of cannabis depenalization after reviewing evidence from a range of countries.

[6]For example, there have been moves over the past decade in California towards more liberal policies related to cannabis. In 2010 California passed into law a depenalization policy that reduced the penalty associated with being found in possession of less than one ounce of cannabis, from a misdemeanor to a civil infraction. Further moves to a more liberal regulation of the cannabis market - almost to the point of legalization - remain on the policy agenda in California [Kilmer *et al.* 2010]. The moves to medical marijuana legislation have also been pronounced, with 17

Anecdotal evidence suggests local support for the scheme began to decline once the policy was announced to have been extended beyond the initial six-month pilot. Media reports cited that local opposition arose due to concerns that children were at risk from the scheme, and that the depenalization policy had increased drug tourism into Lambeth. The LCWS formally ended on 31st July 2002. Post-policy, Lambeth's cannabis policing strategy did not return identically to what it had been pre-policy, partly because of disagreements between the police and local politicians over the policy's true impact. Rather, it adjusted to be a firmer version of what had occurred during the pilot so that police officers in Lambeth continued to issue warnings but would now also have the discretion to arrest where the offence was aggravated.[7] Hence when we refer to measuring the long run impacts of the depenalization policy, we are capturing the total effects arising from: (i) the long run impact of the introduction of the depenalization policy between June 2001 and July 2002; (ii) any longer term differences in policing towards cannabis from the post and pre-policy periods.

The impact of the LCWS depenalization policy on patterns of *crime* in Lambeth and neighboring boroughs is studied extensively by Adda *et al.* [2011]. For the purposes of the current study on the relationship between drug-policing and public health, there are three key results on the impact of the depenalization policy on crime to bear in mind: its impact on the market for cannabis, on the market for Class-A drugs, and drug tourism. First, the LCWS led to a significant and permanent rise in cannabis related criminal offences in Lambeth. Using data on finely disaggregated drug offence type reveals that both the demand for and supply of cannabis are likely to have significantly risen in Lambeth after the introduction of the depenalization policy. This result is important for the current study because it suggests the depenalization policy caused an abrupt, quantitatively large and permanent shock to the cannabis market, causing its equilibrium market size to increase.[8] This will consequently affect the equilibrium market size for Class-A drugs if the markets are related in some way, either because of economies of scale in supplying both drug markets, or because consumer preferences being such that the demands for cannabis and Class-A drugs are either complements or substitutes.

Second, Adda *et al.* [2011] do indeed find evidence of the inter-relatedness between the markets for cannabis and Class-A drugs: they report that the longer term effect of the LCWS was to lead to a significant increase in offences related to the possession of Class-A drugs. However, there is little evidence that the police reallocated their efforts towards crimes relating to Class-A drugs; rather the police appear to have reallocated effort towards non-drug crime. In the current paper we estimate the relationship between the policing of cannabis and hospital admissions related to

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US states currently having such laws in place [Chu, 2012].

[7]Aggravating factors included: (i) if the officer feared disorder; (ii) if the person was openly smoking cannabis in a public place; (iii) those aged 17 or under were found in possession of cannabis; (iv) individuals found in possession of cannabis were in or near schools, youth clubs or child play areas.

[8]Cannabis possession offences increased by 13.5% during the policy, and 24.2% in the post policy period (August 2002 to January 2006) relative to the pre-policy period Adda *et al.* [2011].

Class-A drug use. It is therefore important that changes the reallocation of police resources to other crimes did not counteract any mechanism linking cannabis and Class-A drug consumption. In our analysis when we consider the long term impact of the depenalization policy we take as given the results established in Adda *et al.* [2011] that in response to the policy, the police reallocated effort away from cannabis related crime and towards non-drugs crime; the police did not reallocate effort towards Class-A drug related crime. Given the addictive nature of Class-A drugs, and potential lags in seeking out and receiving treatment, we might also reasonably expect any impact of the LCWS on hospital admissions related to Class-A drug use to last well into the post-policy period. We therefore later consider how the effects of the LCWS on drug-related hospital admissions evolve over time across various cohorts.

The third key finding from Adda *et al.* [2011] on the impact of the LCWS on crime is the existence of drug tourism into Lambeth from its geographic neighboring boroughs. Indeed, these flows into Lambeth explain almost half the increase in cannabis offences in Lambeth. To explore this further in terms of health outcomes, we later investigate whether there are similar geographic spillovers in hospital admission rates related to Class-A admissions after the LCWS is introduced in Lambeth, for those *resident* in neighboring boroughs. Our administrative data on hospital admissions further allow us to shed light on the nature of drug tourists, by exploring how the marginal impact on those resident in neighboring boroughs, differs from the marginal impact on residents of Lambeth.

Standard consumer theory provides clear set of predictions on how such a depenalization policy can impact the use of cannabis and other illicit drugs. Most existing studies assume that such policies cause significant reductions in the price of cannabis [Thies and Register, 1993; Grossman and Chaloupka, 1998; Williams *et al.*, 2004]. This will, all else equal, increase the demand for cannabis in part because of greater demands from *existing* users and also because of an impact on the extensive margin so that new individuals choose to *start* consuming cannabis at the lower price. This will have a positive impact on the consumption of Class-A drugs if cannabis and Class-A drugs are contemporaneously complements in user preferences, or will increase the demand for Class-A drugs over time if the use of cannabis serves as a gateway to the use of other harder illicit drugs. Of course if cannabis and Class-A drugs are substitutes, then the increased demand for cannabis resulting from the depenalization of cannabis possession should reduce Class-A drug use and potentially reduce hospitalizations related to Class-A drug use.[9]

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[9] The administrative records we exploit are not rich enough to estimate a model of consumer demand and then estimate whether cannabis and Class-A drugs are substitutes or complements. Such an exercise is conducted by DiNardo and Lemiuex [2001] for the case of cannabis and alcohol demands using the Monitoring the Future Data in the US.

3 Data, Descriptives and Empirical Method

3.1 Administrative Records on Hospital Admissions

Data on hospital admissions are drawn from the Inpatient Hospital Episode Statistics (HES). These provide an administrative record of every inpatient health episode, defined as a single period of care under one consultant in an English National Health Service hospital.[10] These administrative records are the most comprehensive data source on health service usage for England, and have not been used much by research economists. Inpatients include all those admitted to hospital with the intention of an overnight stay, plus day case procedures when the patient is formally admitted to a hospital bed. As such, these records cover the most serious health events. Patients with less serious conditions receive treatment elsewhere, including outpatient appointments, emergency departments, or primary care services. If such health events are also impacted by drugs policing strategies, our estimates based solely on inpatient records provide a strict *lower* bound impact of the depenalization of cannabis on public health. For each patient-episode event in the administrative records, the data record the date of admission, total duration in hospital, and ICD-10 diagnoses codes in order of importance. Background patient information covers their age, gender, and their zip code of residence at the time of admission.[11]

We assess how hospital admissions related to Class-A drug use and to cannabis use are impacted by the depenalization of cannabis possession in Lambeth. For Class-A drug related admissions, we include episodes where the drug is mentioned either in the primary diagnosis, or those episodes directly caused by Class-A drugs. As hospital admissions for cannabis are far rarer, we include episodes where the drug is mentioned as either a primary or a secondary diagnosis.[12] Given that our main outcome relates to rates of hospital inpatient admissions, we aggregate the individual patient-episode level data by borough of residence and quarter, and calculate admission rates per thousand population for diagnosis d, borough of residence b in quarter q of year y as follows,

$$Admit_{dbqy} = \frac{Tot_{dbqy}}{Pop_{by}}, \tag{1}$$

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[10]We include all episodes of each hospital stay, so that if a patient is under the care of different consultants during their stay in hospital and before discharge, these count as multiple patient-episodes. Given the infrequency with which the same patient transfers across consultants during a hospital stay, the majority of results presented are robust to re-defining episodes at the patient-consultant level.

[11]Between 10 and 12% of the population in England have private health insurance, largely provided by employers. However, this is typically a top-up to NHS care, and does not cover serious illness or most emergencies. Private hospitals do not have emergency rooms, and the use of private primary health care is very rare. The data will therefore capture a very high proportion of adverse drug reactions that require treatment in hospital. The ICD is the international standard diagnostic classification for epidemiological and clinical use.

[12]Diagnoses that mention Class-A drugs include (drug specific) mental and behavioral disorders (ICD-10 Codes F11 for opiods, F14 for cocaine, F16 for hallucinogens), intentional and accidental poisoning (T400-T406 T408-T409, X42, X62 Y12), and the finding of the drug in the blood (R781-R785). Diagnoses that mention cannabis include mental and behavioral disorders (F12), and poisoning (T407).

where Tot_{dbqy} are total number of hospital admissions for diagnosis d, amongst those residing in borough b, in quarter q of year y, and Pop_{by} is the population of borough b in year y. These admission rates are calculated by gender and age cohort, where age is categorized into ten year bins (15-24, 25-34, 35-44) and patient's age is defined as that on the eve of the LCWS policy. For each age-gender cohort, we create a panel of hospital admission rates for all London boroughs, excluding those that neighbor Lambeth (Croydon, Merton, Southwark and Wandsworth). Neighboring boroughs are excluded from our baseline specifications, as evidence from Adda *et al.* [2011] has already demonstrated substantive spillovers in criminal activity (and we later document such policy spillovers in terms of public health). Our data covers hospital admissions among residents of the remaining 28 boroughs (including Lambeth), by quarter, from April 1997 to December 2009.

To reiterate, the geographic information we use relates to the patient's borough of *residence*, not the borough in which they are hospitalized. This helps ameliorate concerns that any changes in Class-A drug related hospitalization rates are driven by changes in the location of hospitals, or changes in drug-related services provided by hospitals.[13] Hence, any documented change in hospital admissions for Class-A drug related diagnoses in Lambeth following the introduction of the LCWS might then operate through two mechanisms: (i) a change in behavior of those resident in Lambeth prior to the policy; (ii) a change in the composition of Lambeth residents, with the policy potentially inducing a net inflow of people into the borough with a higher propensity for Class-A drug use. In Section 4.3 we use our data to shed light on these channels separately, but our baseline estimates, in line with much of the earlier literature, certainly combine both channels.

The administrative records also allow us to create panels based on prior histories of patient admissions because the HES records have unique patient identifiers that allow the same patient to be tracked over episodes between 1997 and 2009. We focus on histories of admissions related to the use of either drugs (Class-A drugs, cannabis, or other illicit drug), or alcohol, and create panels by borough-quarter-age cohort-gender, for those with and without pre-policy histories of admissions related to drugs or alcohol. Among those with no pre-policy admissions, we calculate admission rates as per (1). For those with pre-policy admission rates, Pop_{by} is replaced by the number of distinct individuals admitted for diagnoses related to illicit drugs or alcohol whilst residing in borough b between April 1997 and June 2001, the eve of the LCWS policy in Lambeth.

The depenalization policy likely lowers prices for cannabis in Lambeth, all else equal. For those with no prior history of hospitalization for drug or alcohol use, this might induce greater consumption of Class-A drugs if they are complements to cannabis, or cannabis acts a gateway to such substances. To be clear, among this cohort we pick up the combined impacts among those

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[13]Annual Office for National Statistics (ONS) population estimates at the borough level are only provided in five-year bands [Office for National Statistics, 2011]. As such, the estimates will only record the size of a particular 10-year age cohort once every five years. For example, in 2001, the 25-34 cohort was equal to the population aged 20-24 plus the population 25-29. To deal with this populations are interpolated in all other years, but taking a weighted sum of the relevant cohorts. In 2002, the same cohort were 21-30, and therefore split between three five-year age bins. We therefore interpolate as follows: (0.8× total aged 20-24) + total aged 25-29 + (0.2 × total aged 30-34). Results are robust to fixing the population at 2001 levels.

that *were previously* using illicit drugs (and potentially other substances) but not so heavily so as to induce hospitalizations, as well as those that *begin* to use cannabis and Class-A drugs for the first time as a result of the price impacts on cannabis of the depenalization policy. Among the cohort with histories of hospitalization for drug or alcohol use, there are likely to be long term and heavy users of illicit substances. Such individuals' consumption of Class-A drugs might reasonably be more habitual and so less price sensitive. Hence this cohort might be less impacted by the depenalization of cannabis.

3.2   Cannabis and Class-A Drug Use

Our primary interest is to understand how changes in police enforcement strategies towards the cannabis market - as embodied in the LCWS policy - impacts public health through changes in hospitalization rates related to illicit drug use. Of course the policy would most directly affect the consumption of cannabis, although changes in hospital admissions related to cannabis use are statistically hard to detect given the rarity of such events. On this point, it is instructive to compare rates of drug related hospital admissions from the HES administrative records, to rates of self-reported drug *use* from household surveys the most reliable of which is the British Crime Survey (BCS). Estimates from the BCS in 2002/3 indicate that cannabis was by far the most popular illicit drug, with 16% of 16-24 year-olds and 9% of 25-34 year-olds reporting to have used cannabis in the month prior to the survey. The corresponding figures for Class-A drug use are just 4% and 2% respectively [Condon and Smith, 2003]. The HES records show that there are seven times as many inpatient hospital admissions for Class-A drugs than for cannabis. This reinforces the notion that cannabis related policing policies such as the LCWS, may not lead to a rise in cannabis related hospital admissions even if there is a substantial increase in cannabis usage caused by the policy.

What is important for our analysis is that a body of evidence suggests the cannabis and Class-A drug markets are linked: while little is known about such potential linkages on the supply side, on the demand side this might be because cannabis users are more likely to consume Class-A drugs, both contemporaneously and in the future [van Ours, 2003; Melberg *et al.*, 2010; Bretteville-Jensen *et al.*, 2008; Colea *et al.*, 2004]. There are of course multiple explanations for this positive correlation between admissions for cannabis and subsequent risky behaviors. One explanation is state dependence so that cannabis users have particular characteristics that also lead them to subsequently misuse Class-A drugs, a channel shown to be of first order importance using data from the NLSY97 by Deza [2011]. Alternatively, the use of cannabis might act as a causal "gateway" to the use of harder drugs, as has been suggested by [Beenstock and Rahav, 2002; Bretteville-Jensen *et al.*, 2008; Melberg *et al.*, 2010; van Ours, 2003].

For our study what is important is that some correlation between the market sizes for cannabis and other illicit drugs exists, not whether this is link is causal or not. To show the relatedness

between these markets as recorded in the hospital admissions records we exploit, we present descriptive evidence from the HES to suggest how cannabis consumption today might correlate to Class-A drug use in the future. To do so we exploit the individual identifiers in the administrative records, allowing us to track the same person over time. We then calculate the probability, conditional on an admission in 1997 or 1998, of being readmitted to hospital at least once between 2000 and 2004. Four groups of admission are considered: (i) "cannabis admissions", who were admitted for cannabis, the drug affected by the LCWS; (ii) "Class-A admissions", who were admitted for the use of a harder drug; (iii) "alcohol admissions", who were admitted for alcohol related diagnoses; (iv) "all other admissions", who were admitted for any other cause and serve as a benchmark for the persistence of ill-health over these time periods.[14] Table 1 shows the mean and standard deviation for each probability of readmission, conditional on prior admissions.

Two points are of note. First, there is substantial persistence in hospital admissions for the same risky behavior, as shown on the leading diagonal in Columns 1-3. Persistence is particularly high for Class-A drugs and alcohol, where 26 and 23% of individuals respectively, were readmitted for the ill-effects of the same risky behavior over the two time periods. Reading across the last row of Table 1 on subsequent readmission to hospital from 2000 to 2004 for any diagnosis unrelated to drugs or alcohol, we see that this readmission probability is between 15 and 28% conditional on having been previously admitted in 1997-8 for some risky behavior related to illicit drug or alcohol use. Second, although admissions for any form of risky behavior in 2000-4 is best predicted by admission for the same behavior in 1997-8, we note that for those admitted for Class-A drugs in 2000-4, 5.4% will have been admitted for cannabis related diagnoses in 1997-8. This is significantly higher than having been previously admitted for alcohol related diagnoses (2.2%) over the same period. This highlights the particularly robust correlation between cannabis use at a given moment in time, and future hospital admissions for Class-A related drug use.

In this paper our focus is on establishing whether a change in police enforcement in the cannabis market - as embodied in the LCWS - has a causal impact on hospital admissions for Class-A drugs. The evidence presented in Table 1 and the existing evidence documenting a causal impact of cannabis consumption on the subsequent use of other illicit substances, suggests that as long as the policy affects the usage of cannabis consumption in some way, this is likely to have a knock on effect on the usage of Class-A drugs in the long run. It is these longer term effects on public health that we now focus on measuring.

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[14]As already noted, cannabis related admissions are rare and so in Table 1 we expand the geographic coverage of the sample to cover metropolitan local authorities in Greater Manchester, Merseyside, the West Midlands, Tyne and Wear, and South Yorkshire, in addition to London that our main analysis is based on. This sample accounts for approximately 30% of England's population. We exclude Lambeth from this analysis to prevent any impact of the LCWS contaminating these results. For Class-A drug admissions, we include episodes that mention Class-A drugs as either a primary or secondary diagnosis, as the objective is to assess correlations in drug use, not the cause of admission. We exclude those admitted for more than one risky behavior related to cannabis, Class-A drugs and alcohol. Finally, observations for 1999 are dropped to ensure that we only capture new incidents between 1997-8 and the later time period.

3.3 Empirical Method and Descriptive Evidence

To estimate the impact of the depenalization policy on hospital admissions rates, we estimate the following balanced panel data specification for diagnosis d in London borough b in quarter q and year y,

$$Admit_{dbqy} = \alpha + \beta_0 P_{qy} + \beta_1 [L_b \times P_{qy}] + \beta_2 PP_{qy} + \beta_3 [L_b \times PP_{qy}] + \delta X_{bqy} + \lambda_b + \lambda_q + u_{bqy}, \quad (2)$$

where $Admit_{dbqy}$ is the of the number of admissions to hospital where the primary diagnosis relates to Class-A drugs, per thousand of the population, as defined in (1). P_{qy} and PP_{qy} are dummies for the policy and post-policy periods respectively and L_b is a dummy for the borough of Lambeth. The specification is estimated separately for each age-gender cohort. The parameters of interest are estimated using a standard difference-in-difference (DD) research design: β_1 and β_3 capture differential changes in hospital admission rates in Lambeth during and after the depenalization policy period, relative to other London boroughs.

β_0 and β_2 capture London-wide time trends in admissions. In particular, β_0 captures London-wide trends in hospitalization rates occurring at the same time as the LCWS was in operation in Lambeth. β_2 captures longer term London-wide trends in hospitalization rates after the depenalization policy in Lambeth officially ends. This coefficient picks up any impacts on hospitalization rates related to diagnosis-d for London and nationwide policies, including the nationwide depenalization of cannabis possession that occurred from January 2004 through to January 2009.[15] Although there are of course expected to be strong London-wide trends in hospitalization rates, our research design identifies whether: (i) hospitalization rates in Lambeth significantly diverge away from London-wide trends during and after the depenalization policy is in place; (ii) these divergences precisely coincide with the depenalization policy's operation in Lambeth.

In X_{bqy} we control for two sets of borough-specific time varying characteristics. The first contains the shares of the population under 5 and over 75 (by borough and year), who place the heaviest burden on health services. Second, X_{bqy} includes controls for admission rates, by borough-quarter-cohort, for conditions that should be unaffected by the LCWS, in particular malignant neoplasms, diseases of the eye and ear, diseases of the circulatory system, diseases of the respiratory system, and diseases of the digestive system. These capture contemporaneous changes in healthcare

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[15] The seeds of the nationwide decriminalization policy were sown in October 2001 – during the initial six month phase of the LCWS – when the then Home Secretary, David Blunkett, asked the Advisory Council on the Misuse of Drugs (ACMD) to review the legal classification of cannabis within the UK's three-tiered system. In March 2002 ACMD recommended cannabis be declassified to a Class-C drug, because the existing Class-B classification was, "disproportionate in relation both to its inherent toxicity, and that of other substances...currently within class B". In March 2002 the Parliamentary Home Affairs Select Committee supported such a declassification and cannabis was formally declassified from a Class-B drug to a Class-C drug in the UK on January 29th 2004. This declassification effectively decriminalized the possession of small quantities of cannabis for personal use, mirroring the LCWS policy experiment. Like the LCWS, the nationwide policy would be reversed – on January 26th 2009 as concerns grew over the potential links between cannabis use and mental health, and changes in the composition of psychoactive ingredients in cannabis supply.

provision or levels of illness in the population that could affect drug-related admissions. The admission rates for these diagnoses are all constructed from the HES administrative records. The fixed effects capture remaining permanent differences in admissions by borough (λ_b) and quarter (λ_q). Observations are weighed by borough shares of the London-wide population. Defining t as quarters since April 1997: $t = [4 \times (y - 1997)] + q$, we assume a Prais-Winsten borough specific AR(1) error structure, $u_{bqy} = u_{bt} = \rho_b u_{bt-1} + e_{bt}$, where e_{bt} is a classical error term. u_{bqy} is borough specific heteroskedastic, and contemporaneously correlated across boroughs.[16]

Table 2 provides descriptive evidence on the unconditional long term effects of the depenalization policy on Class-A related hospital admissions rates, with each row showing hospital admissions rates ($Admit_{dbqy}$) as defined in (1). We focus first on male cohorts of various ages on the eve of the LCWS policy. Columns 1 and 2 present means and standard deviations of hospital admission rates related to Class-A drug usage in Lambeth during the pre-policy and post-policy periods respectively; Columns 3-4 give the corresponding statistics for the average borough in the rest of London (excluding Lambeth's neighboring boroughs). We note that in the pre-policy period Lambeth had substantially higher rates of admissions than the London average. Indeed, ranking boroughs by their per-policy hospital admission rates related to Class-A drugs, Lambeth has the third highest for men and second highest for women. However, as shown later, there is no evidence of diverging or converging trends in Class-A related hospital admissions rates between Lambeth and the London average in the pre-policy period from 1997 to 2001. In Lambeth, admissions rates in the pre-policy period are lowest for the youngest cohort, reflecting the overall pattern of drug admissions by age.

Comparing Columns 1 and 2 begins to highlight the potential health impacts of the depenalization policy: hospital admission rates in Lambeth rise over time for the 15-24 and 25-34 cohorts, but fall slightly for the oldest cohort. In contrast for the rest of London admissions rates rise only for the youngest cohort and are stable or declining for the older two age cohorts. Columns 5-6 then present difference-in-difference estimates of how Class-A drug admissions rates relate to the LCWS policy. Column 5 shows that unconditional on any other factor, admission rates for both the 15-24 and 25-34 cohorts significantly rose in Lambeth relative to the London borough average, after the introduction of the policy to depenalize the possession of cannabis. The relative increases in admission rates of .054 and .079 per thousand population for the youngest two age cohorts are statistically significant at the 5% level: the increases correspond to a 146% rise relative to the pre-policy level for the 15-24 cohort, and a 44% increase above the baseline level for the cohort aged 25-34 on the eve of the policy. The effect for the oldest cohort is not statistically significantly different from zero. Column 6 then shows this basic pattern of difference-in-differences to remain in magnitude and significance once borough and quarter year fixed effects are controlled for. These

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[16]While we think it is important to try and control for the general state of health within the borough using the variables described in X_{bqy}, our main results are robust to excluding such controls. We also note that estimating AR(1) error terms is the most conservative approach: allowing standard errors to be clustered either by borough or by borough-year leads to far smaller estimates of standard errors for the main results.

results suggest that among younger male age cohorts, the policy of depenalizing the possession of cannabis is associated with significantly higher hospitalization rates in Lambeth for Class-A drug use in the longer term.

Table A1 shows the corresponding results for female age cohorts: we find no significant impacts on Class-A related hospitalization for any female age cohort. The rate of admissions for such diagnoses among women is generally lower than among men and this might be one reason it is harder to statistically measure any impact. On the other hand, the fact that there are very different trends in hospitalizations for Class-A drugs across genders within Lambeth, suggests the earlier results for men are not merely picking up other changes in hospital behavior or how diagnoses are recorded, that might otherwise have been expected to impact men and women equally.

Table A2 shows the corresponding descriptive evidence for hospital admissions related to cannabis use for male cohorts. As already discussed, cannabis hospital admission rates are generally lower than for Class-A drugs, especially among older age cohorts, despite much higher levels of cannabis usage as suggested by survey data. The difference-in-difference results suggest the LCWS had no significant impact on hospital admissions for cannabis: the point estimates for the youngest male cohorts are positive but not precisely estimated, and a similar set of findings are obtained when examining the impact of the depenalization policy on hospitalizations for cannabis related diagnoses among female cohorts (not shown).

To relate these findings to the literature, recall that Model [1993] find that the *de facto* decriminalization of cannabis in twelve US states from the mid-1970s significantly increased cannabis-related emergency room admissions. Chu [2012] similarly finds that the passage of US state laws that allow individuals to use cannabis for medical purposes leads to a significant increase in referred treatments to rehabilitation centres. Our evidence from London suggests that if a similar effect occurs from the depenalization of cannabis possession, it does not then feed through to significantly higher rates of hospitalization that involve an overnight stay, which is what our inpatient administrative data measures. For the bulk of our remaining analysis, we therefore continue to focus on Class-A hospital admissions among male cohorts of various ages.

4 Baseline Results

4.1 The Impact of the LCWS by Cohort

Table 3 presents estimates of the full baseline specification (2), where we consider the impact of the LCWS on Class-A drug related hospital admissions rates for three male age cohorts in Columns 1 to 3. These findings represent our core results: they show that the addition of time varying local authority controls makes very little difference to the unconditional difference-in-difference estimates presented in Table 2: the first row shows that in the longer term post-policy period, there are statistically significant rises in admission rates of between 3 and 6% for the youngest two

cohorts in Lambeth, relative to other non-neighboring London boroughs. In line with the earlier descriptive evidence, no impact of the policy is found on the oldest age cohort, that were aged 34-44 on the eve of the cannabis depenalization policy being introduced in Lambeth.

The second row of Table 3 shows that in the short-run, during the 13 months in which the LCWS was actually in operation, there are no statistically significant effects on hospitalization rates for two of the three cohorts, and among those aged 25-34 hospitalization rates are actually declining during the policy period. Hence, as might be expected, any impact of the cannabis depenalization policy on hospitalization rates for Class-A drug use takes time to work through. The other coefficients in Table 3, estimates of β_0 and β_2, show that for London on average, there are no significant long-term time trends in admission rates during and after the policy period for the older two cohorts, once other correlates of admissions rates are controlled for. For the youngest cohort in Column 1, hospital admission rates are rising over time but the results overall show that hospitalization rates in Lambeth are diverging away from this London wide average in the post-policy period, all else equal.[17]

Our results therefore suggest the depenalization of cannabis led to longer term increases in the use of Class-A drugs and subsequent hospitalizations related to Class-A drug use among the younger two age cohorts. If the depenalization policy led to a decline in the equilibrium price of cannabis in Lambeth, as is often argued to be an unambiguous effect of such policies [Kilmer *et al.*, 2010], then this result suggests that cannabis and Class-A drugs have a negative cross-price elasticity, so that the two types of illicit drug are contemporaneous complements, or the use of cannabis serves as gateway to the later use of harder illicit drugs.[18] This is in line with other studies that have estimated the cross-price elasticity between cannabis and an specific Class-A drug: cocaine - either using decriminalization as a proxy for a price reduction [Thies and Register, 1993; Grossman and Chaloupka, 1998], or using actual price information [Williams *et al.*, 2004].

In Table 4 we seek to exploit the richness of our administrative records to examine the long run policy impacts on age cohorts further subdivided by their histories of hospital admission for drug and alcohol related diagnoses during the pre-policy period from April 1997 to June 2001. This allows us to shed light on whether those prior record of substance abuse resulting in hospital admission respond differentially to the depenalization of cannabis than does the rest of the population.[19] Relative to the existing literature linking drug enforcement policies and health, this allows us to present novel evidence on the characteristics of the marginal individuals most impacted by

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[17]As mentioned earlier, estimating AR(1) error terms is the most conservative approach: allowing standard errors to be clustered either by borough or by borough-year leads to far smaller estimates of standard errors for these baseline results. This pattern of significant policy impacts across male age cohorts is robust to using slightly different dependent variable measures of hospitalizations related to Class-A drug use: (i) the absolute number of admissions (Tot_{dbqy}); (ii) the log of the number of Class-A related admissions per 1000 of the population plus one $Ln\left(\frac{Tot_{dbqy}}{Pop_{by}}+1\right)$.

[18]No reliable information on the price of illicit drugs exists at the borough level for our study period.

[19]During this pre-policy period 9368 individuals were admitted to hospital for drugs or alcohol (primary or non-primary): 710 in the 15-24 cohort; 2709 among those 25-34; and 5949 among those aged 35-44.

a policy of depenalizing cannabis. Our coefficients of interest remain the differential impact over time of the policy in Lambeth relative to the rest of London.

Columns 1 to 3 of Table 4 consider admissions among male each age cohort for those *without* a prior record of admissions. The evidence suggests that for all age cohorts, there are significant increases in Class-A drug related hospitalizations in Lambeth relative to the rest of London in the post-policy period relative to pre-policy. The impacts are large, ranging from a .042 increase in the admission rate per thousand among the youngest cohort to a 0.19 increase among the oldest cohort of men that were aged 35-44 on the eve of the policy. We find no evidence of significant increases in hospitalization rates among each age cohort in the very short run when the LCWS policy is actually in place (and the point estimates are each smaller than the longer term impacts in Lambeth): as is intuitive, this suggests that any increase in Class-A drug use as a result of the policy takes time to work through to increased hospitalization rates.

The London-wide trends in admissions rates shown in the third row of Columns 1 to 3 reflect how this samples is defined: admission rates for those without previous admissions must necessarily rise (weakly) over time given they start at zero and cannot be negative. The data suggests that this upward trend is significantly more pronounced in Lambeth post-policy across all age cohorts.

The remaining Columns in Table 4 then repeat the estimation for each age cohort among those that have a prior history of at least one hospitalization for drug or alcohol related diagnoses. These borough-quarter-year aggregates are therefore constructed from fewer individual patients (ranging from 1,709 individuals in the 15-24 age cohort, 4,397 in the 25-34 age cohort, and 6,165 in the oldest age cohort). The results suggests that in the longer term such cohorts are either not affected by the depenalization policy, or their admission rates decline in the long term.[20] However, relative to the results in Columns 1-3, some of the non-significance of the findings on the main coefficient of interest, β_1, for the cohorts with histories of hospital admissions, are driven by the coefficient being less precisely estimated. This imprecision might reflect the greater heterogeneity among populations that have histories of hospitalization for drug and alcohol use. Moreover, given that most individuals begin using illicit drugs early in life, those in older cohorts with histories of hospitalizations for drug and alcohol use are likely to be long term drug users. Hence on the margin, such long term drug users appear to be much less clearly impacted by the depenalization of cannabis in terms of further hospital admissions for Class-A drug use.[21] In terms of standard consumer theory, such long term users might be more habituated in their behavior and less price sensitive to any change in price of cannabis induced by the depenalization policy.

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[20]This downward trend among the specifications based on those with admissions histories partially reflects the fact that not all such individuals are admitted more than once. Of the 12271 individuals admitted for drugs or alcohol related diagnoses in the pre-policy period, only 56% (6871 individuals) have a second admission at any point during the sample period, and only 38% (4684 individuals) have another episode in the policy or post-policy period, and this naturally induces a downward time trend to be picked up in β_0 and β_2.

[21]This finding is also consistent with the evidence based on NLSY97 data in Deza [2011] who uses a dynamic discrete choice model to document that the gateway effect from cannabis to hard drugs use is weaker among older age cohorts.

In summary the evidence suggests that there are quantitatively large impacts of the the police policy of depenalizing cannabis on public health, as measured in hospitalization rates for Class-A related drug use, and that a significant portion of this increase is concentrated among individuals that have no prior history of hospitalization for drug or alcohol related diagnoses. To be clear, these results cannot be interpreted as suggesting that there are some individuals that *start* taking Class-A drugs as a result of the depenalization of cannabis. All we can infer is that those that have no prior history of hospital admissions related to illicit drugs or alcohol, be it because they were not consuming illicit drugs, or were consuming them in moderation, are significantly impacted by the depenalization policy.

4.2   Robustness Checks

We now present robustness check on our principal findings. An obvious concern with these results is that they might in part be confounded by natural time trends in hospitalizations for Class-A drugs. These time trends might also differ across age groups and by hospital admissions histories. To directly address this, we repeat the analysis but augment (2) with controls for borough specific linear time trends. Table 5 presents the results, again broken down for cohorts based on age and prior admissions histories.[22]

The inclusion of borough specific linear time trends serves to reinforce the earlier conclusions. For the specifications in Columns 1-3 by age cohort among those with no prior history of hospitalization for drug or alcohol related diagnoses, we continue to find significant increases in hospitalization rates in the long run in Lambeth relative to the rest of London. The magnitudes of these point estimate impacts are in fact *larger* among each age cohort than was reported in Table 4 when time trends were not controlled for. Hospitalization rates significantly increase by .07 among the youngest cohort aged 15-24 on the eve of the policy, increase significantly by .17 among those aged 25-34, and increase significantly by .30 for the oldest age cohort with no history of hospitalizations for drug and alcohol use.

Columns 4-6 show the impacts among age cohorts with a prior history of hospitalization for drug or alcohol related diagnosis. In these sub-populations we find no significant longer term increases for any of the age cohorts. Taken together these results reinforce the notion that in the longer term, the impacts of the policy on public health are very much concentrated among those that, prior to the policy, had no history of hospitalization for alcohol or drug related diagnoses.[23]

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[22]For the specifications in Columns 1-3 of Table 5 based on samples without a prior record of hospital admissions the time trend is set to zero pre-policy and allowed to be linear thereafter ($\lambda_b \times$quarters post Q3 2001). For the specifications by age cohort with a history of admissions in Columns 4-6 of Table 5, the borough specific time trend is assumed to be linear over the sample period.

[23]Again the coefficients on the policy period and post-policy dummy variables, β_0 and β_2, reflect the way in which the cohorts are defined by hospitalization history: for those with no prior history of hospitalizations, hospitalization rates must necessarily (weakly) rise over time; among those with a prior history of hospitalization, hospitalization rates must necessarily fall as long as not every individual in this cohort is re-admitted to hospital in the post-policy period.

A second way to address the concern of whether the results are in part driven by differential time trends in hospitalization rates is to exploit the four years of panel data *prior* to the introduction of the depenalization policy. Specifically, we use this period to test whether within the pre-policy period there is any evidence of a divergence in trends in hospitalization rates between Lambeth and the rest of London. We therefore estimate (2) but additionally control for an interaction between the Lambeth dummy and a dummy set equal to one from mid way during the pre-policy period (Q1 2000) until the actual start of the policy (Q2 2001), and zero otherwise. The sample stops in Q3 2001 when the LCWS policy is actually initiated. As Table A3 shows, for all male age cohorts and admissions histories, this placebo dummy interaction is not found to be significantly different from zero suggesting that hospitalization rates in Lambeth are not diverging from London in the years prior to the depenalization policy. As discussed in Section 2, this is very much in line with the evidence related to the underlying motivation behind why the policy was introduced, that emphasized the ability of the police to reallocate their effort towards non-cannabis crime, and which hardly mentioned the potential impacts on public health. Hence the data supports the assertion that the depenalization policy was not introduced specifically into Lambeth because of concerns over worsening public health related to drug-related hospital admissions. Nor is there any evidence of reversion to the mean in hospitalization rates with Lambeth converging back towards London-wide averages.

A second robustness check addresses the concern that the OLS specifications estimated do not account for the censoring in the data: indeed defining the dependent variable as in equation (1) we treat those borough-quarter-year observations in which there are zero admissions the same in which there are strictly positive numbers of admissions. We address this by re-estimating (2) using a Tobit model that allows us to estimate the impact of the policy on both the extensive margin (i.e. the probability that there is at least one admission in a given borough-quarter) and the intensive margins (the admission rate per borough-quarter, conditional on at least one admission). However, the introduction of non-linearity means the difference-in-difference coefficient no longer equals the marginal effect of the interaction term [Ai and Norton, 2003]. Policy impacts are therefore produced by using our Tobit estimates to calculate the average interaction term for $PP_{qy} \times Lambeth$ and $P_{qy} \times Lambeth$.[24] Estimated policy effects on the extensive and intensive margins are presented in Table A4 by male age cohort. In line with the baseline results in Table 3, we see that the policy leads to a statistically significant increase in admission rates on the intensive margin, that is an increase in the admission rate conditional on at least one admission per borough quarter, for the two youngest age cohorts. On the extensive margin, namely the probability of a positive admission

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[24]Following Buis [2010], given that both interacted variables are binary, the average interaction effect on each margin can be calculated by: first, using the Tobit estimates to produce the conditional expected value of admissions for the four Lambeth x policy period (PP_{qy} or P_{qy}) cells (e.g, Lambeth = 0, PP_{qy}=0; Lambeth = 1, PP_{qy} = 0; Lambeth = 0, PP_{qy} = 1; Lambeth = 0, PP_{qy} = 0); and, second, taking the double difference of those conditional expected admission rates. The average interaction effect for the intensive margin in the post-policy period is therefore equal to the following:

rate, the impact is positive but not statistically significant except for the oldest cohort.

4.3 Interpretation

Throughout the analysis we have used the borough of residence at the time of admission to build hospitalization rates across cohorts. The documented increase in hospital admissions for Class-A drug related diagnoses in Lambeth following the introduction of the LCWS might then operate through two mechanisms: (i) a change in behavior of those resident in Lambeth prior to the policy; (ii) a change in the composition of Lambeth residents, with the policy inducing a net inflow of people into the borough with a higher propensity for Class-A drug use. Undoubtedly, the geographical distances between London boroughs are small and travel costs are low relative to the fixed costs of permanently changing residence. However, if drug users perceive the depenalization of cannabis in Lambeth as signaling a wider weakening of police enforcement against all illicit drugs, there might be longer term benefits to relocating to the borough. Our results pick up the combined effect of both channels, but for policy design it is useful to understand whether both channels are indeed at play. In this subsection we therefore try to shed some light on the extent to which our results might be explained by the migration of drug users into Lambeth from other parts of London as a result of the depenalization policy.[25]

The HES data contain information on borough of residence for each individual admission to hospital, with individual identifiers allowing us to link patients across episodes and time. The major limitation of using hospital administrative records to shed light on changes in borough of residence in response to the policy, is that for those that are admitted *only once* during the study period, the data does not allow us to identify whether they have changed residence over time prior to the admission, or will do so subsequent to the admission. These individuals, that form the bulk of hospital admissions and that are included in the main analysis, cannot be included in the analysis below examining migration patterns. While this obviously limits our ability to shed light on the potential net migration into Lambeth of drug users in response to the depenalization policy, we know of no data set representative at the London borough level, that would match both changes in residence over time with individual hospital admissions or health outcomes over time.

$$
\begin{aligned}
\widehat{\beta_3} = (\widehat{E}[AR_{qyb}|PP_{qy} = 1, L_b = 1, \lambda_b, \lambda_q, \lambda_y, X_{bqy}, AR_{qyb} > 0] - \\
\widehat{E}[AR_{qyb}|PP_{qy} = 0, L_b = 1, \lambda_b, \lambda_q, \lambda_y, X_{bqy}, Admits_{qyb} > 0]) - \\
(\widehat{E}[AR_{qyb}|PP_{qy} = 1, L_b = 0, \lambda_b, \lambda_q, \lambda_y, X_{bqy}, Admits_{qyb} > 0] - \\
\widehat{E}[AR_{qyb}|PP_{qy} = 0, L_b = 0, \lambda_b, \lambda_q, \lambda_y, X_{bqy}, Admits_{qyb} > 0]) \quad (3)
\end{aligned}
$$

where the conditional expected values are taken over all observations and then averaged. The corresponding difference-in-difference coefficient on the extensive margin (the probability of a non-zero admission rate) can be calculated analogously. The exercise is repeated for the policy-period.

[25]We thank Jonathan Caulkins and Libor Dusek for comments that have motivated this subsection.

We therefore proceed by documenting changes in borough of residence for those that have at least two admissions into hospital between 1997 and 2007. To get a sense of the sample selection this induces, we note that in the pre-policy period, 326,683 men are admitted into hospital for any diagnosis, of which 10.6% are re-admitted (at least once) somewhere in London during the one-year period in which the LCWS policy is in place, and 25.3% are re-admitted (at least once) anytime in the post-policy period. Among those 1,746 individuals admitted for Class-A drug related diagnosis in the pre-period, only 14.7% are observed being re-admitted for any diagnosis during the policy period, and 28.2% are observed being re-admitted for any diagnosis during the post-policy period.

If individuals are induced to migrate to Lambeth in response to the depenalization policy, they might do so at some point during its actual period of operation between June 2001 and July 2002. To check for this, we first focus on those 1,630 individuals that are admitted to hospital for any diagnosis in Lambeth during the policy period, and that are observed having at least one prior hospital admission somewhere in London pre-policy. Of these 1,630 individuals, 1.7% are admitted for Class-A related diagnosis in Lambeth during the policy period. These are perhaps the most likely individuals to have moved to Lambeth in specific response to the depenalization policy. However we note that among this group, almost all their earlier pre-policy admissions (for any diagnosis) occur in Lambeth, so that there is no strong evidence of these individuals having recently moved to Lambeth during the policy period.

While these results focus on those admitted for Class-A drug related diagnosis in Lambeth during the policy period, it might well be the case that drug users that migrate into Lambeth because of the policy are first admitted for some other diagnosis. Hence, we next focus on the 98.3% of hospital admissions in Lambeth during the policy period for any diagnosis unrelated to Class-A drug usage. Among these individuals, nearly all of them are observed with all their earlier admissions in Lambeth; only 10.3% have their last prior admission in some other borough, indicating that they changed their borough of residence at some point between their last admission and the end of the policy period. Taken together, these two pieces of evidence show that among those men with at least two hospital admissions since 1997, there is very limited evidence of there being significant changes of residence into Lambeth during the formal policy period between June 2001 and July 2002.

Our next set of results examines longer term patterns of changes in borough residence. Given the fixed costs of changing residence and that in the post-policy period policy enforcement in Lambeth remained somewhat different than other boroughs, it might be reasonable to assume that a net inflow of drug users into Lambeth simply takes some time to occur. To check for this we examine whether inflows into Lambeth from other London boroughs change between two four-year windows: the first four year window occurs entirely pre-policy from April 1997 to April 2001, and the second four year window occurs entirely post-policy from April 2003 to April 2007. In each window we check whether among those admitted to hospital at least twice in the four-year window, and, where at least one admission relates to a diagnosis indicating Class-A drug use,

whether changes in borough of residence between the first and last admission vary over time.

In the first four-year pre-policy window from 1997-2001 we observe: (i) of those that have their first admission outside of Lambeth, 1.4% are observed with a later admission in Lambeth; (ii) of those that have their first admission in Lambeth, 16% are observed with a later admission outside of Lambeth. Doing the same for the later four-year window from 2003-7 to see if this pattern of migration is altered in the longer term, we find that: (i) of those that have their first admission outside of Lambeth, 3.0% are observed with a later admission in Lambeth; (ii) of those that have their first admission in Lambeth, 30% are observed with a later admission outside of Lambeth. Hence there is evidence of more frequent changes of residence among this subsample post-policy, but that this increase occurs both into Lambeth and from Lambeth: the inflow into Lambeth from other boroughs in the post-policy window relative to the pre-policy window increases (3.0% relative to 1.8%), but this is offset by the percentage increase in outflows from Lambeth to other boroughs among such individuals (30% relative to 16%).[26] Overall this suggests is that, among those with multiple hospital admissions, there is increased mobility of residents across boroughs over time, but there is no strong evidence of systematically increased inflows into Lambeth over the second four year window relative to the first.

5 Extended Results

In this Section we consider three margins of policy impact in more detail: the dynamic response, spillover effects into neighboring boroughs driven by drug tourism (rather than permanent changes in residence), and the severity of hospital admissions. Establishing the existence and magnitude of each effect is important to feed into any assessment of the overall social costs of the change in drug enforcement policy.

5.1 The Dynamics of the Response

When investigating how the impact of the depenalization policy on hospitalizations for Class-A drugs evolves over time, our objectives are two-fold: to assess how long the change in police enforcement took to filter through to hospital admissions, and whether, and how quickly, those effects eventually die out. To chart the time profile of responses, we replace the post-policy period indicator in (2), PP_{qy}, with three 2-year time-bins: 1-2 years post reform (Q3 2002 to Q2 2004, TB^1); 3-4 years post reform (Q3 2004 to Q2 2006, TB^2); and, 5-6 years post reform (Q3 2006 to

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[26]One additional strategy we considered to shed light on changes in residence induced by the policy. First, we considered using the administrative records on outpatients, that would include visits to general practitioners and local health clinics. Such events are far more common than hospital admissions. However such data only reliably exists in the post-policy period from 2006/7, and contains no information on diagnosis.

Q2 2008, TB^3),

$$Admit_{dbqy} = \alpha + \beta_0 P_{qy} + \beta_1[L_b \times P_{qy}] + \sum_{k=1}^{3}(\mu_k TB_{qy}^k + \gamma_k[L_b \times TB_{qy}^k])$$

$$+ \delta X_{bqy} + \lambda_b + \lambda_q + u_{bqy} \quad (4)$$

where all other variables are as previously defined. This specification is estimated for each 10-year age cohort by pre-policy drugs or alcohol admission history. Impacts of LCWS on admission rates in Lambeth, in each time period (β_1, γ_1, γ_2, and γ_3), are then plotted in Figures 1A and 1B. Figure 1A shows that for those who were not admitted for drugs or alcohol in the pre-policy period, there is a similar inverse-U shaped pattern of dynamic responses across age cohorts. For each cohort the depenalization policy has no impact during the policy period, estimated impacts increase thereafter for some time before starting to decline. In line with the evidence in Tables 4 and 5, the magnitudes of the impacts are largest for those in the older cohorts aged 25-34 and 35-44: for these two age cohorts, the policy impacts are over .20 three to four years into the post-policy period: this corresponds to more than a 100% increase in hospitalization rates relative to the pre-policy baseline levels in Lambeth for the 25-34 age cohort, and a 55% increase for the 35-44 age cohort. In the final period considered, 4-6 years into the post-policy period, we see that there remain significant policy impacts among all three age cohorts, although the policy impact is trending downwards so that eventually the impact would be expected to go to zero.

In comparison to the literature linking policies to regulate the market for illicit drugs and public health, these dynamic responses are of significant duration. For example, Dobkin and Nicosia [2009] study the impact of a government program designed to reduce the supply of methamphetamine on hospitalization rates (by targeting precursors to methamphetamine), as well as other outcomes. This policy is sometimes claimed to have been the DEA's greatest success in disrupting the supply of an illicit drug in the US and indeed Dobkin and Nicosia [2009] find that the policy had significant impacts on public health. However, they document that these effects were short lived: within 18 months admissions rates had returned to pre-intervention levels. In contrast, the depenalization policy we document has an impact on hospitalization rates that lasts at least 5-6 years post-policy for many cohorts even though the policy itself is only in place for a year.

The dynamic impacts among those who have been previously hospitalized for a drug or alcohol related diagnosis in the pre-policy period are very different, as shown in Figure 1B. In line with the regression results shown with and without time trends in Tables 4 and 5 for these cohorts, there are few policy impacts among these cohorts as measured by hospitalization rates related to Class-A drug use.

These dynamic results confirm further reinforce the earlier finding that the impacts of the depenalization of cannabis possession on hospitalization rates related to Class-A drug use, are in the longer term concentrated among those with no prior history of hospitalizations for such drug

use. The impacts on hospitalizations related Class-A drug us take a year or two to emerge after the policy if first initiated, and among cohorts with no prior history of admissions for Class-A drug use or alcohol use, the policy impacts remain above pre-intervention levels six years after the depenalization policy is first introduced.

5.2 Spillovers in Neighboring Boroughs

As discussed earlier, when studying the impacts on crime of the LCWS policy, Adda *et al.* [2011] provide detailed evidence that the depenalization of cannabis in Lambeth induced a substantial degree of drug tourism from geographically neighboring boroughs into Lambeth. Indeed such drugs tourism can explain around half the estimated long run increase in cannabis possession offences within Lambeth. We analyze whether there are similar spillover effects on public health in Lambeth's neighbors, in terms of hospital admission rates for Class-A related drugs. Our administrative data on hospital admissions further allows us to shed light on the nature of drug tourists, by exploring how the marginal individual impacted in neighboring boroughs differs from the marginal individual resident in Lambeth itself.

To do so we augment (2) with interactions between the policy and post-policy period and whether the borough (of residence) is a geographic neighbor ($N_l = 1$) or not ($N_l = 0$).[27] As the characteristics of drug tourists might correlate to their pre-policy hospital admissions history, we find it informative to again split the results by age and admissions history cohorts.

Table 6 presents the results. The first two rows correspond the policy impacts in Lambeth and follow the same patterns of significance and magnitudes as previously discussed in Table 4. The third row, showing the coefficient on the interaction term between the policy-policy dummy and the dummy for whether the borough is a neighbor to Lambeth ($N_l = 1$) is the coefficient of interest for understanding whether there are spillover impacts on public health from the depenalization policy. In Columns 1 to 3 we see that among those without pre-policy admissions histories, there are significant increases in admissions rates in neighboring boroughs for the 25-34 and 35-44 age cohorts. As might be expected, the magnitude of the impacts are smaller in neighboring boroughs than in Lambeth itself: for the 25-34 cohort, the estimated .074 rise in neighboring boroughs is still significantly smaller than the corresponding increase in Lambeth (.142). The impact for the 35-44 cohort is around four times larger in Lambeth (.195) than in neighboring boroughs (.047), with the difference being statistically significant at the 1% level as shown at the foot of Table 6. As with the results within Lambeth, we find no evidence that hospital admissions rates for Class-A drug us in neighboring boroughs rise during the short run when the depenalization policy is actually in place in Lambeth.

Columns 4 to 6 then focus on those with histories of hospitalization for drug or alcohol related diagnoses. As with the findings in Lambeth, the impacts among those cohort - across all age

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[27]Boroughs that neighbor Lambeth are Southwark, Croydon, Wandsworth and Merton.

groups - are not statistically different from zero for the two younger age cohorts, and negative for the oldest age cohort. As shown at the foot of Table 6, for all cohorts we cannot reject the null that the long run policy impact in Lambeth differs from that in neighboring boroughs.

Taken together these results suggest that among those that reside in neighboring boroughs, the drug tourists that are induced to travel to Lambeth as a result of the depenalization of cannabis are more likely to be those that have no prior history of hospitalization for drug or alcohol: these cohorts include those that are likely to have never consumed cannabis or consumed it in small quantities. This is the same cohort of individuals that we previously found to be most directly affected among residents of Lambeth. Reassuringly, the magnitudes of these spillovers in public health are smaller than the direct impacts documented within Lambeth. Among those cohorts whose prior involvement is risky behavior has been extreme enough to result in hospital admission for diagnoses related to illicit drugs or alcohol, relatively fewer individuals are induced to travel to Lambeth as a result of cannabis being depenalized there. This is closely in line with the direct results for Lambeth presented in Tables 4 and 5.

5.3 Severity of Hospital Admissions

A third dimension along which to consider policy impacts relates to the severity of hospitalizations, as measured by the number of days the individual is required to stay in hospital for. This margin is of policy relevance because it maps directly into the resultant healthcare costs associated with the depenalization of cannabis, as calculated below in Section 5.4. We therefore first document how the length of *individual* hospital episodes for diagnoses related to Class-A drug use changes differentially between Lambeth and other London boroughs post-policy relative to the pre-policy period. To do so, we estimate a specification analogous to (2) but where the dependent variable is the individual length of hospital stay in days and the sample is confined to episodes where the primary diagnoses relates to Class-A drugs. We focus on the first episode for any hospital stay (that is the same as the entire hospital stay for 93% of observations), and to avoid the results being driven by outliers, we drop observations where the length of the stay is recorded to be longer than 100 days (that excludes a further 2% of all stays).[28]

Table 7 presents the results, again split by age cohort. The data suggests that in the longer term post-policy, across all three age cohorts, the length of stay significantly increases in Lambeth relative to the London average. For example, among the 15-24 age cohort, hospital stays increased by 3.7 days, and this is relative to a baseline pre-policy hospital stay length of 7.2 days, an increase of 49%. The proportionate changes for the other age cohorts are 29% for the 25-34 age cohort and 20% for the oldest age cohort. Hence, the proportionate changes in length of hospital stay are greater for age cohorts that were younger at the time the depenalization policy was

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[28]These results remain robust to having the dependent variable specified in logs so that outliers are less likely to drive the estimated impacts.

introduced. This emphasizes that quite apart from the impacts of the depenalization of cannabis on hospitalization rates for Class-A diagnosis that has been the focus of our analysis so far, the policy also has impacts on the severity of those admissions for Class-A drug use. Both margins are relevant for thinking through the public health costs of the policy as detailed in the next subsection.

Finally, the coefficients in the third row of Table 7 show that in other London boroughs there are *negative* time trends in the duration of such individual hospitalizations conditional on all other controls in (2). Hence the findings for Lambeth post-policy do not appear to be driven by some systematic lengthening of hospital stays for such diagnosis that might be occurring more generally across London.

5.4   The Public Health Costs of the Depenalization Policy

Our final set of results attempt to provide a lower bound estimate of the public health costs to Lambeth associated with the depenalization policy, as measured exclusively through hospitalizations related to Class-A drug usage. This combines the earlier unconditional estimates from Table 2 (that do not differ much from the baseline estimates in Table 3) on changes in the number of individuals being hospitalized for such diagnosis, and the results from Table 7 showing the policy impacts on the length of hospital episodes, holding constant hospitalization rates related to Class-A diagnoses. Combining the evidence on both margins allows us to infer the overall increase in hospital bed-days related to Class-A drug use attributable to the depenalization policy. More specifically, the change in average hospital-bed days from the pre to the post policy periods, per quarter for residents of borough b in cohort c is given by,

$$\triangle H_{bc} \;\; = \;\; N_{post,bc}\overline{L_{post,bc}} \; - \; N_{pre,bc}\overline{L_{pre,bc}} \tag{5}$$

where $N_{post,bc}$ represents the number of admissions per quarter in the post period in borough b for cohort c, and $\overline{L_{post,bc}}$ is the average length of stay of those that are admitted in this group in the post-policy period; $N_{pre,bc}$ and $\overline{L_{pre,bc}}$ are of course analogously defined over the pre-policy period. Rearranging (5), the change in hospital bed-days can be decomposed as occurring through two channels,

$$\triangle H_{bc} = (N_{post,bc} - N_{pre,bc})\overline{L_{pre,bc}} + N_{post,bc}(\overline{L_{post,bc}} - \overline{L_{pre,bc}}) \tag{6}$$

The first channel represents the policy impact occurring through a change in the number of hospital admissions for Class-A diagnoses, holding constant the length of stay fixed at the pre-reform levels. The $(N_{post,bc} - N_{pre,bc})$ term can be straightforwardly derived from the unconditional baseline estimates presented in Table 2. The second channel represents the policy impact through a change

in the average length of hospital stays, holding constant admission numbers at the post-policy level. The $\overline{(L_{post,bc}} - \overline{L_{pre,bc}})$ corresponds exactly to the estimates reported for each cohort in Table 7. The total public health cost of the policy is then $\triangle H_{bc}$ multiplied by the cost of a hospital bed-day.

We use published National Health Service estimates of the cost per hospital bed-day.[29] This cost is comprised largely of hospital ward costs (nursing, therapies, basic diagnostics and overheads), hence there is actually little variation by diagnosis: the average cost per additional bed day across all adult inpatient diagnoses categories is £240 [Department of Health, 2012], but the upper end of the hospital bed-day costs, relating to those for adult acute (inpatient) mental health stays, are only slightly higher at £295 [PSSRU, 2011]. We therefore use a figure between these estimates, of £250 per hospital bed-day, as quoted by the NHS Institute [NHS Institute, 2012]. This likely represents a lower bound of the true cost of a hospital bed-day because it does not include any specific treatment costs or the additional costs from any associated stay in intensive care.

We then take our estimates from Tables 2 and 7 for each age cohort to calculate each component of (6). For the youngest age cohort of those 15-24 on the eve of the depenalization policy, $\triangle H_{bc}$ =27.2 bed-days per quarter; of this total change, 24.1 bed-days operate through the first channel of increased hospitalization rates, and 4.1 bed-days through the second channel of longer hospital stays conditional on admission. Among those aged 25-34 on the eve of the policy, $\triangle H_{bc}$ =52.5 bed-days per quarter, where the first channel corresponds to an increase of 42.6 bed-days, and the second component generates an increase of 9.9 bed-days. Finally, for the oldest cohort of 35-44 year olds, $\triangle H_{bc}$ =-26.9 bed-days per quarter, where the point estimate in the third row of Table 2 implies a decrease in hospitalization rates post-policy of 30.7 bed-days (although this point estimate was not statistically different from zero), and this is only partially offset by the increase through the second channel of 3.8 bed-days.

Applying the estimated costs per hospital bed-day of £250 to the change in the total number of hospital bed-days per quarter, for each cohort in Lambeth in the post-policy period on average, reveals the increased public health cost to be: (i) £6,802 among those aged 15-24 on the eve of the policy; (ii) £13,136 among those aged 25-34 on the eve of the policy. Summing across four quarters we derive the conservative public health cost of the depenalization policy to be £79,752 per annum, on average across all the post-policy years in the sample.

There are a number of ways this monetary amount can be benchmarked. One way to do this would be relative to health costs in Lambeth as a whole. However, there are multiple components of health costs related to preventative and curative care, and it is unclear which subset of these costs provide the most appropriate benchmark. Moreover, in England health expenditures stem from both local borough sources but also expenditures of the national government. Given these

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[29]Since 2003/4, hospitals have been paid a price or "tariff" for each patient, based on the diagnosis group. These are based on the average stay for the diagnosis group. Additional days spent in hospital are paid at a daily rate. Prior to 2003/4, including the period covered by the LCWS, funding was not as strongly linked to patient numbers or diagnoses.

complications, perhaps the more transparent method through which the benchmark the public heath costs of the depenalization policy is to compare it to the *London-wide* time trends in hospital bed-days, by cohort. This provides a sense of the increased public costs through natural rises over time in hospital bed-days for hospitalizations related to Class-A drug use that would have to be borne between the pre and post policy periods absent the depenalization policy. More precisely this London-wide time trend for cohort c is given by,

$$\triangle H_c = (N_{post,c} - N_{pre,c})\overline{L_{pre,c}} + N_{post,c}(\overline{L_{post,c}} - \overline{L_{pre,c}}), \tag{7}$$

where $(N_{post,c} - N_{pre,c})$ can be derived from the coefficient on the post-policy dummy presented in the unconditional estimates in Table 2, and $\overline{(L_{post,c} - L_{pre,c})}$ is measured from the coefficient on the post policy dummy in Table 7. For the youngest age cohort of those 15-24 on the eve of the depenalization policy, $\triangle H_c =$3.79 bed-days per quarter; of this total change, -4.7 bed-days operate through the first channel of increased hospitalization rates, and 8.5 bed-days through the second channel of longer hospital stays conditional on admission. Among those aged 25-34 on the eve of the policy, $\triangle H_c =-3.5$ bed-days per quarter, where the first channel corresponds to a decrease of 6.5 bed-days, and the second component generates an increase of 3.0 bed-days. Finally, for the oldest cohort of 35-44 year olds, $\triangle H_{bc} =-2.4$ bed-days per quarter, where the first channel corresponds to an increase of 2.6 bed-days, and the second component generates a decrease of 5.2 bed-days.

Aggregating these cohorts across four quarters then suggests the natural *decrease* in costs associated with hospital bed-days is £4,935. Hence the increase in bed-days attributable to the policy more than offsets this natural decrease in hospital bed-days attributable to London wide time trends.

Of course, this calculation still underestimates the total public costs of the increased hospital bed days within Lambeth due to the policy because of the existence of many additional channels that we have ignored. First, we have ignored any additional demands placed on other parts of the national health service unrelated to hospital inpatient stays, as a result of the depenalization policy. These include demands through outpatient appointments, hospital emergency departments, and through treatment centres. Indeed, the existing evidence from the US on the link between the availability of cannabis and health relate to emergency or treatment costs: Model [1993] find that the *de facto* decriminalization of cannabis in twelve US states from the mid-1970s significantly increased cannabis-related emergency room admissions. Chu [2012] similarly finds that the passage of US state laws that allow individuals to use cannabis for medical purposes leads to a significant increase in referred treatments to rehabilitation centres. Second, we have ignored any cost to individual users of being hospitalized. Such events almost surely impact individual welfare, especially given the robust association found across countries in the gradient between health and life satisfaction.

6 Discussion

We evaluate the impact of a policing experiment that depenalized the possession of small quantities of cannabis in the London borough of Lambeth, on hospital admissions related to illicit drug use. Despite health costs being a major social cost associated with markets for illicit drugs, evidence on the link between how such markets are regulated and public health remains scarce. Our analysis provides novel evidence on this relationship, at a time when many countries are debating moving towards more liberal policies towards illicit drugs markets, with the depenalization or decriminalization of cannabis possession as the most often suggested or implemented policy. We have exploited administrative records on individual hospital admissions classified by ICD-10 diagnosis codes, that have previously not been much used by economists. We use these records to construct a quarterly panel data set by London borough running from 1997 to 2009 to estimate the short and long run impacts of the depenalization policy unilaterally introduced in Lambeth between 2001 and 2002.

We find the depenalization of cannabis had significant longer term impacts on hospital admissions related to the use of hard drugs. Among Lambeth residents, the impacts are concentrated among men in younger age cohorts, and among those with no prior history of hospitalization related to illicit drug or alcohol use. The dynamic impacts across cohorts vary in profile with some cohorts experiencing hospitalization rates remaining above pre-intervention levels six years after the depenalization policy is first introduced. We find evidence of smaller but significant positive spillover effects in hospitalization rates for hard drug use among residents in boroughs neighboring Lambeth, and these are again concentrated among younger cohorts without prior histories of hospitalizations related to illicit drug or alcohol use. We combine these estimated impacts on hospitalization rates with estimates on how the policy impacted the severity of hospital admissions to provide a lower bound estimate of the public health cost of the depenalization policy.

Our analysis contributes to the nascent literature evaluating the health impacts of changes in enforcement policies in the market for illicit drugs. The depenalization of cannabis is one of the most common forms of such policy either implemented (such as in the Netherlands, Australia and Portugal) or being debated around the world (such as in many countries in Latin America). The practical way in which the depenalization policy we study was implemented is very much in line with policy changes in other countries that have changed enforcement strategies in illicit drug markets and as such we expect our results to have external validity to those settings. However unlike those settings, we are able to exploit a (within-city) borough level intervention and so estimate the policy impacts using a difference-in-difference design, as well as exploring differential impacts across population cohorts, where cohorts are defined by gender, age, previous admissions history, and borough of residence. This is different from much of the earlier research that, with the exception of studies based on US or Australian data, can typically only study nationwide changes in drug enforcement policies such as depenalization, and have therefore had to rely on time variation

alone to identify policy impacts [Reuter, 2010]. The administrative records we exploit allow us to provide novel evidence on how the impacts of such policies vary across population cohorts, over time within a cohort, how they interact with potential changes of residence of drug users, and how drug tourism might be induced without permanent changes in residence.

Clearly, such policy impacts are unlikely to ever be estimated using randomized control trial research designs. We have used a difference-in-difference research design exploiting an unusual policy experiment in one London borough that allows us to exploit within and across borough differences in health outcomes to identify policy impacts. The key concern with such a research design is to distinguish policy impacts from time trends. To do so, we have used the detailed administrative records to present evidence on how different cohorts (by gender, age and previous admissions history) are differentially impacted by the policy, how the results are strengthened when controlling for time trends, and checked for the presence of trends in the pre-policy period.

Our results suggest policing strategies have significant, nuanced and lasting impacts on public health. In particular our results provide a note of caution to moves to adopt more liberal approaches to the regulation of illicit drug markets, as typically embodied in policies such as the depenalization of cannabis. While such policies may well have numerous benefits such as preventing many young people from being criminalized (around 70% of drug-related criminal offences relate to cannabis possession in London over the study period), allowing the police to reallocate their effort towards other crime types and indeed reduce total crime overall [Adda *et al.*, 2011], there remain potentially offsetting costs related to public health that also need to be factored into any cost benefit analysis of such approaches.

Two further broad points are worth reiterating. First, our analysis relates to the more general study of the interplay between the consumption of different types of drug. In particular there is a large literature testing for the "gateway hypothesis" that the consumption of one "soft" drug causally increases the probability of subsequently using a "harder drug". The crucial challenge for identification is the potential for unobserved factors or heterogeneity that could drive consumption of multiple types of drug. Existing work has tried to tackle this problem by either: (i) instrumenting the gateway drug with a factor unrelated to the underlying heterogeneity, typically using cigarette and alcohol prices [Pacula, 1998; Beenstock and Rahav, 2002; DiNardo and Lemiuex, 2001]; or, (ii) using econometric techniques to model the possible effects of unobserved heterogeneity [van Ours, 2003; Pudney, 2003; Melberg *et al.*, 2010]. To be clear, in our analysis we make no attempt to test for gateway effects directly, but our contribution to this literature is to demonstrate that the markets for cannabis and hard drugs are concretely linked - be it because of gateway effects or some other channel - so that changes in policy that affect one market will have important repercussions for the other [DeSimone and Farrelly, 2003; van Ours and Williams, 2007; Bretteville-Jensen *et al.*, 2008].

Finally, our analysis highlights the impact that policing strategies can have on public health more broadly. It is possible that other policing strategies, such as police visibility or zero-tolerance

policies, could also have first order implications for public health. These effects could operate through a multitude of channels including: (i) police behavior directly impacting markets and activities that determine individual health, such as the case studied in this paper; (ii) police behavior affecting perceptions of crime and thus influencing psychic well-being. This possibility opens up a rich area of further study at the nexus of the economics of crime and health.

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Table 1: Hospital Re-admission Probabilities
Means, standard deviations in parentheses, standard errors in square brackets

	Admitted in 1997 or 1998 for:			
	(1) Cannabis Related Diagnoses	(2) Class-A Drug Related Diagnoses	(3) Alcohol Related Diagnoses	(4) All Other Diagnoses
Admitted in 2000-2004 for:				
Cannabis Related Diagnoses	.092	.011	.005	.001
	(.289)	(.105)	(.071)	(.034)
Class-A Related Diagnoses	.054	.257	.022	.004
	(.227)	(.440)	(.145)	(.061)
Alcohol Related Diagnoses	.060	.064	.225	.015
	(.238)	(.245)	(.418)	(.121)
All Other Diagnoses	.283	.146	.208	.316
	(.451)	(.353)	(.409)	(.465)
Observations (individuals)	533	3950	15595	485992

Notes: The figures refer to the probability of re-admission as a hospital inpatient over the period 2000 to 2004, conditional on an earlier hospital admission in 1997 or 1998. Class-A drugs include cocaine, opioids, and hallucinogens. For each type of admission related to a risky behavior (Class-A drugs, cannabis, alcohol) , we include episodes that mention this substance as either a primary or secondary (any further) diagnosis. We exclude a small number of cases for those admitted for more than one behavior related to cannabis, Class-A drugs and alcohol in 1997 or 1998. The sample is based on all male individuals aged 10-39 on 1st July 2001, the eve of the LCWS policy. The sample is drawn from all London boroughs, except Lambeth, plus all unitary authorities Greater Manchester, Merseyside, the West Midlands, Tyne and Wear, and South Yorkshire, in addition to London. The total numbers admitted between 2000 and 2004 - irrespective of admission history - are as follows: all other diagnoses, 1325795; cannabis 3446; Class A drugs, 14105; alcohol, 53033. Totals exclude those admitted for more than one drug/alcohol related diagnosis.

Table 2: Class-A Drug Related Hospital Admission Rates for Male Cohorts, by Borough and Time Period

Means, standard deviations in parentheses, standard errors in square brackets

	Lambeth		Rest of London		Post-Policy Minus Pre-Policy	
	(1) Pre-Policy	(2) Post-Policy	(3) Pre-Policy	(4) Post-Policy	(5) Unconditional	(6) Fixed Effects
Men aged 15-24	.037	.131	.028	.069	.054**	.054**
	(.067)	(.082)	(.049)	(.074)	[.022]	[.022]
Men aged 25-34	.179	.259	.084	.086	.079**	.080**
	(.103)	(.127)	(.094)	(.086)	[.034]	[.034]
Men aged 35-44	.362	.311	.069	.061	-.039	-.039
	(.122)	(.186)	(.088)	(.080)	[.065]	[.065]
Observations (b-q-y)	17	30	459	810	-	-

Notes: *** denotes significance at 1%, ** at 5%, and * at 10% level. The dependent variable is the number of male Class-A drug related hospital admissions per 1000 of the male population in the cohort, where the primary diagnosis refers to a Class-A drug. Class-A drugs include cocaine, opioids, and hallucinogens. All observations are at the borough-quarter-year level. Observations are at the quarter-borough-year level and are weighted by population of the borough relative to the population of London. In Columns 1 and 3 the pre-policy period runs from Q1 1997 to Q2 2001. The policy period runs from Q3 2001 to Q2 2002. In Columns 2 and 4 the post-policy period runs from Q3 2001 to Q4 2009. In Columns 3 and 4 the sample is based on all London boroughs excluding Lambeth and boroughs neighbouring Lambeth. In Columns 5 and 6, standard errors on differences are calculated assuming a Prais-Winsten borough specific AR(1) error structure, that allows for borough specific heteroskedasticity and error terms to be contemporaneously correlated across boroughs. In Column 6, the differences are calculated from regression specification that also controls for borough and quarter fixed effects. Male age cohorts are defined by age on the eve of the introduction of the LCWS policy, 1st July 2001.

Table 3: The Impact of the LCWS by Male Age Cohort

Dependent Variable: Male Hospital Admission Rates for Class-A Drug Related Diagnoses

	(1) Aged 15-24	(2) Aged 25-34	(3) Aged 35-44
Post-Policy x Lambeth	.0380*	.0749**	-.0339
	(.0229)	(.0334)	(.0626)
Policy Period x Lambeth	.0282	-.0288	-.156
	(.0396)	(.0606)	(.104)
Post-Policy	.0289***	-.00715	.000513
	(.00609)	(.00707)	(.00766)
Policy-Period	.00986	-.0227***	-.0123
	(.00765)	(.00775)	(.00892)
Borough and Quarter Fixed Effects	Yes	Yes	Yes
Demographic Controls	Yes	Yes	Yes
Adjusted R-squared	.256	.395	.435
Observations (borough-quarter-year)	1,428	1,428	1,428

Notes: *** denotes significance at 1%, ** at 5%, and * at 10% level. The dependent variable the number of Class-A drug related hospital admissions per 1000 of the population in the cohort where the primary diagnosis refers to a Class-A drug. Class-A drugs include cocaine, opioids, and hallucinogens. All observations are at the borough-quarter-year level. The sample period runs from Q2 1997 until Q4 2009. Control boroughs are all other London boroughs, excluding Lambeth's neighbours (Croydon, Merton, Southwark and Wandsworth). Panel corrected standard errors are calculated using a Prais-Winsten regression, where a borough specific AR(1) process is assumed. This also allows the error terms to be borough specific heteroskedastic, and contemporaneously correlated across boroughs. Observations are weighted by the share of the total (excluding neighbouring boroughs) London population that year in the borough. The Policy-Period dummy variable is equal to one from Q3 2001 to Q2 2002, and zero otherwise. The Post-Policy dummy is equal to one from Q3 2002 onwards, and zero otherwise. Column 1 relates to admissions of those aged 15-24 on 1st July 2001. Column 2 relates to admissions of those aged 25-34 on 1st July 2001. Column 3 relates to admissions of those aged 35-44 on 1st July 2001. All specifications include borough and quarter fixed effects, and control for shares of the population aged under 5 and over 75 at the borough-year level, and borough-quarter-year level admissions for malignant neoplasm, diseases of the eye and ear, diseases of the circulatory system, diseases of the respiratory system, and diseases of the digestive system. These admission rates are derived from the HES administrative records at the borough-quarter-year level.

Table 4: The Impact of the LCWS by Male Age Cohort and Admission history

Dependent Variable: Male Hospital Admission Rates for Class-A Drug Related Diagnoses

	Aged 15-24	Aged 25-34	Aged 35-44	Aged 15-24	Aged 25-34	Aged 35-44
Age Cohort:	No	No	No	Yes	Yes	Yes
Pre-Policy Drugs or Alcohol Admissions:	(1)	(2)	(3)	(4)	(5)	(6)
Post-Policy x Lambeth	.0423***	.137***	.187***	3.903	-2.055	-8.115***
	(.0143)	(.0329)	(.0395)	(4.142)	(1.469)	(1.304)
Policy Period x Lambeth	.0240	.0482	.0317	1.787	-1.149	-5.666**
	(.0242)	(.0543)	(.0654)	(7.247)	(2.868)	(2.379)
Post-Policy	.0545***	.0665***	.0539***	-5.634***	-9.851***	-4.788***
	(.00473)	(.00552)	(.00499)	(.519)	(.567)	(.421)
Policy-Period	.0394***	.0517***	.0422***	-6.695***	-9.289***	-4.788***
	(.00601)	(.00633)	(.00572)	(.933)	(.938)	(.682)
Borough and Quarter Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Demographic Controls	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted R-squared	.327	.413	.381	.181	.413	.367
Observations (borough-quarter-year)	1,428	1,428	1,428	1428	1428	1428

Notes: *** denotes significance at 1%, ** at 5%, and * at 10% level. The dependent variable is the number of Class-A drug related hospital admissions per 1000 of the population in the cohort where the primary diagnosis refers to a Class-A drug. Class-A drugs include cocaine, opioids, and hallucinogens. All observations are at the borough-quarter-year level. The sample period runs from Q2 1997 until Q4 2009. Control boroughs are all other London boroughs, excluding Lambeth's neighbours (Croydon, Merton, Southwark and Wandsworth). Panel corrected standard errors are calculated using a Prais-Winsten regression, where a borough specific AR(1) process is assumed. This also allows the error terms to be borough specific heteroskedastic, and contemporaneously correlated across boroughs. Observations are weighted by the share of the total (excluding neighbouring boroughs) London population that year in the borough. The Policy-Period dummy variable is equal to one from Q3 2001 to Q2 2002, and zero otherwise. The Post-Policy dummy is equal to one from Q3 2002 onwards, and zero otherwise. Columns 1 and 4 relate to admissions of those aged 15-24 on 1st July 2001. Columns 2 and 5 relate to admissions of those aged 25-34 on 1st July 2001. Columns 3 and 6 relate to admissions for those aged 35-44 on 1st July 2001. All specifications include borough and quarter fixed effects, and control for shares of the population aged under 5 and over 75 at the borough-year level, and borough-quarter-year level admissions for malignant neoplasm, diseases of the eye and ear, diseases of the circulatory system, diseases of the respiratory system, and diseases of the digestive system. All these admission rates are also derived from the HES administrative records at the borough-quarter-year level.

Table 5: Time Trends

Dependent Variable: Male Hospital Admission Rates for Class-A Drug Related Diagnoses

Age Cohort:	Aged 15-24	Aged 25-34	Aged 35-44	Aged 15-24	Aged 25-34	Aged 35-44
Pre-Policy Drugs or Alcohol Admissions:	No	No	No	Yes	Yes	Yes
	(1)	(2)	(3)	(4)	(5)	(6)
Post-Policy x Lambeth	.0651***	.172***	.300***	3.183	-2.357	-6.417***
	(.0188)	(.0427)	(.0489)	(5.637)	(1.962)	(1.497)
Policy-Period x Lambeth	.0250	.0411	.0578	1.854	-.702	-6.604***
	(.0243)	(.0517)	(.0602)	(7.133)	(2.857)	(1.959)
Post-Policy	.0361***	.0637***	.0494***	-6.420***	-9.655***	-4.295***
	(.00420)	(.00570)	(.00512)	(.862)	(.787)	(.565)
Policy-Period	.0367***	.0512***	.0395***	-6.815***	-9.220***	-4.803***
	(.00465)	(.00610)	(.00570)	(.954)	(.893)	(.649)
Borough and Quarter Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Demographic Controls	Yes	Yes	Yes	Yes	Yes	Yes
Borough Specific Post-Policy Linear Time Trend	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted R-squared	.391	.446	.449	.170	.400	.404
Observations (borough-quarter-year)	1428	1428	1428	1428	1428	1428

Notes: *** denotes significance at 1%, ** at 5%, and * at 10% level. The dependent variable is the number of Class-A drug related hospital admissions per 1000 of the population in the cohort where the primary diagnosis refers to a Class-A drug. Class-A drugs include cocaine, opioids, and hallucinogens. All observations are at the borough-quarter-year level. The sample period runs from Q2 1997 until Q4 2009. Control boroughs are all other London boroughs, excluding Lambeth's neighbours (Croydon, Merton, Southwark and Wandsworth). Panel corrected standard errors are calculated using a Prais-Winsten regression, where a borough specific AR(1) process is assumed. This also allows the error terms to be borough specific heteroskedastic, and contemporaneously correlated across boroughs. Observations are weighted by the share of the total (excluding neighbouring boroughs) London population that year in the borough. The Policy-Period dummy variable is equal to one from Q3 2001 to Q2 2002, and zero otherwise. The Post-Policy dummy is equal to one from Q3 2002 onwards, and zero otherwise. Columns 1 and 3 relate admissions of those aged 15-24 on 1st July 2001. Columns 2 and 5 relate to admissions of those aged 25-34 on 1st July 2001. Columns 3 and 6 relate to admissions for those aged 35-44 on 1st July 2001. Columns 1-3 control for post Q3 2001 borough specific linear time trends. Columns 4-6 control for borough specific linear time trends. All Columns include borough and quarter fixed effects, and control for shares of the population aged under 5 and over 75 at the borough-year level, and borough-quarter-year level admissions for malignant neoplasm, diseases of the eye and ear, diseases of the circulatory system, diseases of the respiratory system, and diseases of the digestive system. All these admission rates are also derived from the HES administrative records at the borough-quarter-year level.

Table 6: Impacts in Neighboring Boroughs

Dependent Variable: Male Hospital Admission Rates for Class-A Drug Related Diagnoses

Age Cohort:	Aged 15-24	Aged 25-34	Aged 35-44	Aged 15-24	Aged 25-34	Aged 35-44
Pre-Policy Drugs or Alcohol Admissions:	No	No	No	Yes	Yes	Yes
	(1)	(2)	(3)	(4)	(5)	(6)
Post-Policy x Lambeth (PP x L)	.0362**	.142***	.195***	3.870	-2.536*	-8.186***
	(.0145)	(.0314)	(.0440)	(4.142)	(1.467)	(1.180)
Policy Period x Lambeth (P x L)	.0229	.0457	.0534	1.807	-1.109	-6.227***
	(.0248)	(.0529)	(.0720)	(7.191)	(2.885)	(2.174)
Post-Policy x Neighbours (PP x N)	.000756	.0740***	.0469***	-1.749	.234	-3.618***
	(.00849)	(.0167)	(.0135)	(1.162)	(1.296)	(.702)
Policy-Period x Neighbours (P x N)	-.0106	-.00274	.0164	-3.351*	-1.464	-3.496***
	(.0147)	(.0259)	(.0224)	(2.001)	(2.378)	(1.200)
Post-Policy (PP)	.0519***	.0651***	.0563***	-5.657***	-9.957***	-4.821***
	(.00451)	(.00575)	(.00504)	(.524)	(.573)	(.421)
Policy-Period (P)	.0380***	.0515***	.0442***	-6.643***	-9.380***	-4.845***
	(.00585)	(.00654)	(.00588)	(.962)	(.957)	(.682)
Testing Differences between Lambeth and Her Neighbors						
(PP x L) - (PP x N)	.0355*	.0675**	.148***	5.16	-2.77	-4.57
	(.0185)	(.0333)	(.0421)	(7.43)	(1.67)	(1.50)
(P x L) - (P x N)	.0335*	.0484	.0370	1.08	.355	-2.73
	(.0326)	(.0556)	(.0687)	(.717)	(3.27)	(2.10)
Borough and Quarter Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Demographic Controls	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted R-squared	.331	.449	.382	.158	.351	.399
Observations (borough-quarter-year)	1,632	1,632	1,632	1,632	1,632	1,632

Notes: *** denotes significance at 1%, ** at 5%, and * at 10% level. The dependent variable is the number of Class-A drug related hospital admissions per 1000 of the population in the cohort where the primary diagnosis refers to a Class-A drug. Class-A drugs include cocaine, opioids, and hallucinogens. Observations are at the quarter-borough-year level and are weighted by population of the borough relative to the population of London. Panel corrected standard errors are calculated using a Prais-Winsten regression, where a borough specific AR(1) process is assumed. This also allows the error terms to be borough specific heteroskedastic, and contemporaneously correlated across boroughs. The sample period runs from Q2 1997 to Q4 2009. The Policy-Period dummy variable is equal to one from Q3 2001 to Q2 2002, and zero otherwise. The Post-Policy dummy is equal to one from Q3 2002 onwards, and zero otherwise. Columns 1 and 4 relate to admissions of those aged 15-24 on 1st July 2001. Columns 2 and 5 relate to admissions of those aged 25-34 on 1st July 2001. Columns 3 and 6 relate to admissions for those aged 35-44 on 1st July 2001. The Neighbors dummy variable is equal to one when the local authority neighbors Lambeth (Croydon, Merton, Southwark and Wandsworth), and zero otherwise. All columns include borough and quarter fixed effects, and control for shares of the population aged under 5 and over 75 at the borough-year level, and borough-quarter-year level admissions for malignant neoplasm, diseases of the eye and ear, diseases of the circulatory system, diseases of the respiratory system, and diseases of the digestive system. All these admission rates are also derived from the HES administrative records at the borough-quarter-year level.

Table 7: Impacts on Length of Hospital Stay

Dependent Variable: Length of Hospital Stay in Days for Males Admitted with Drug-Related Diagnoses

	(1) Aged 15-24	(2) Aged 25-34	(3) Aged 35-44
Post-Policy x Lambeth	3.72***	3.49***	2.38***
	(1.34)	(.820)	(.570)
Policy Period x Lambeth	.077	4.80***	-6.24***
	(1.28)	(1.09)	(1.53)
Post-Policy	-2.74*	-2.43***	-1.97***
	(1.34)	(.798)	(.615)
Policy-Period	-.030	-.297	1.98
	(1.39)	(1.10)	(1.54)
Borough and Quarter Fixed Effects	Yes	Yes	Yes
Demographic Controls	Yes	Yes	Yes
Adjusted R-Squared	.083	.098	.071
Observation (individual hospital episode)	1374	2810	1806

Notes: *** denotes significance at 1%, ** at 5%, and * at 10% level. The dependent variable is the number of days spent in hospital (discharge date - admission date) amongst those admitted to hospital for Class A drugs. Observations are at the episode or admission level. The sample includes only the first episode of a hospital stay (93% of all episodes) and episodes lasting less than 100 days (excluding 160 or 2% of episodes across all years and cohorts). Standard errors are clustered at the borough level. The sample period runs from January 1997 to December 2009. The Policy-Period dummy variable is equal to one from Q3 2001 to Q2 2002, and zero otherwise. The Post-Policy dummy is equal to one from Q3 2002 onwards, and zero otherwise. All columns include borough and quarter fixed effects, and control for borough-quarter-year level admissions for malignant neoplasm, diseases of the eye and ear, diseases of the circulatory system, diseases of the respiratory system, and diseases of the digestive system. All these admission rates are also derived from the HES administrative records at the borough-quarter-year level.

Table A1: Class-A Drug Related Hospital Admission Rates for Female Cohorts, by Borough and Time Period

Means, standard deviations in parentheses, standard errors in square brackets

	Lambeth		Rest of London		Post-Policy Minus Pre-Policy	
	(1) Pre-Policy	(2) Post-Policy	(3) Pre-Policy	(4) Post-Policy	(5) Unconditional	(6) Fixed Effects
Women aged 15-24	.060	.098	.016	.033	.021	.021
	(.067)	(.079)	(.047)	(.066)	[.024]	[.024]
Women aged 25-34	.159	.149	.037	.038	-.010	-.009
	(.090)	(.071)	(.059)	(.057)	[.023]	[.023]
Women aged 35-44	.116	.116	.023	.021	-.003	-.003
	(.071)	(.082)	(.044)	(.041)	[.028]	[.028]
Observations (b-q-y)	17	30	459	810	-	-

Notes: *** denotes significance at 1%, ** at 5%, and * at 10% level. The dependent variable the number of female Class-A drug related hospital admissions per 1000 of the female population in the age cohort where the primary diagnosis refers to a Class-A drug. Class-A drugs include cocaine, opioids, and hallucinogens. All observations are at the borough-quarter-year level. Observations are at the quarter-borough-year level and are weighted by population of the borough relative to the population of London. In Columns 1 and 3 the pre-policy period runs from Q1 1997 to Q2 2001. The policy period runs from Q3 2001 to Q2 2002. In Columns 2 and 4 the post-policy period runs from Q3 2001 to Q4 2009. In Columns 3 and 4 the sample is based on all London boroughs excluding Lambeth and boroughs neighbouring Lambeth. In Columns 5 and 6, standard errors on differences are calculated assuming a Prais-Winsten borough specific AR(1) error structure, that allows for borough specific heteroskedasticity and error terms to be contemporaneously correlated across boroughs. In Column 6, the differences are calculated from regression specification that also controls for borough and quarter fixed effects. Male age cohorts are defined by age on the eve of the introduction of the LCWS policy, 1st July 2001.

Table A2: Cannabis Related Hospital Admissions for Male Cohorts, by Borough and Time Period
Means, standard deviations in parentheses, standard errors in square brackets

	Lambeth		Rest of London		Post-Policy Minus Pre-Policy	
	(1) Pre-Policy	(2) Post-Policy	(3) Pre-Policy	(4) Post-Policy	(5) Unconditional	(6) Fixed Effects
Men aged 15-24	.055	.108	.025	.083	.001	.001
	(.075)	(.072)	(.055)	(.094)	[.018]	[.018]
Men aged 25-34	.083	.088	.025	.047	-.018	-.017
	(.087)	(.057)	(.044)	(.060)	[.027]	[.027]
Men aged 35-44	.125	.093	.018	.039	-.046	-.044
	(.136)	(.078)	(.044)	(.064)	[.041]	[.040]
Observations (borough-quarter-year)	17	30	558	930	-	-

Notes: *** denotes significance at 1%, ** at 5%, and * at 10% level. The dependent variable is the number of hospital admissions per thousand population where the diagnosis (primary or secondary) refers to cannabis. Observations are at the quarter-borough-year level and are weighted by population of the borough relative to the population of London. In Columns 1 and 3 the pre-policy period runs from Q1 1997 to Q2 2001. The policy period runs from Q3 2001 to Q2 2002. In Columns 2 and 4 the post-policy period runs from Q3 2001 to Q4 2009. In Columns 3 and 4 the sample is based on all London boroughs excluding Lambeth. In Columns 5 and 6, standard errors on differences are calculated assuming a Prais-Winsten borough specific AR(1) error structure, that allows for borough specific heteroskedasticity and error terms to be contemporaneously correlated across boroughs. In Column 6, the differences are calculated from regression specification that also controls for borough and quarter fixed effects. Male age cohorts are defined by age on the eve of the introduction of the LCWS policy, 1st July 2001.

Table A3: Placebo Pre-Policy Check

Dependent Variable: Male Hospital Admission Rates for Class-A Drug Related Diagnoses

	(1) Aged 15-24	(2) Aged 25-34	(3) Aged 35-44
2nd Half Pre-Policy x Lambeth	.0216	-.00403	-.00509
	(.0278)	(.0356)	(.0468)
2nd Half Pre-Policy	.0122**	.0103	.00206
	(.00494)	(.00838)	(.0123)
Borough and Quarter Fixed Effects	Yes	Yes	Yes
Demographic Controls	Yes	Yes	Yes
Adjusted R-squared	.187	.393	.680
Observations (borough-quarter-year)	476	476	476

Notes: *** denotes significance at 1%, ** at 5%, and * at 10% level. The dependent variable the number of Class-A drug related hospital admissions per 1000 of the population in the cohort where the primary diagnosis refers to a Class-A drug. Class-A drugs include cocaine, opioids, and hallucinogens. All observations are at the borough-quarter-year level. The sample period runs from Q2 1997 until Q2 2001. Control boroughs are all other London boroughs, excluding Lambeth's neighbours (Croydon, Merton, Southwark and Wandsworth). Panel corrected standard errors are calculated using a Prais-Winsten regression, where a borough specific AR(1) process is assumed. This also allows the error terms to be borough specific heteroskedastic, and contemporaneously correlated across boroughs. Observations are weighted by the share of the total (excluding neighbouring boroughs) London population that year in the borough. The 2nd Half Pre dummy variable is equal to one from Q2 1999 to Q2 2001, and zero otherwise. Column 1 relates to admissions of those aged 15-24 on 1st July 2001. Column 2 relates to admissions of those aged 25-34 on 1st July 2001. Column 3 relates to admissions of those aged 35-44 on 1st July 2001. All specifications include borough and quarter fixed effects, and control for shares of the population aged under 5 and over 75 at the borough-year level, and borough-quarter-year level admissions for malignant neoplasm, diseases of the eye and ear, diseases of the circulatory system, diseases of the respiratory system, and diseases of the digestive system. All these admission rates are also derived from the HES administrative records at the borough-quarter-year level.

Table A4: Tobit Specifications

Dependent Variable: Male Hospital Admission Rates for Class-A Drug Related Diagnoses

Robust standard errors in parentheses

	Margin: Extensive	Intensive	Extensive	Intensive	Extensive	Intensive
Age Cohort:	Aged 15-24		Aged 25-34		Aged 35-44	
	(1)	(2)	(3)	(4)	(5)	(6)
Post-Policy x Lambeth	.150	.0434***	.0133	.0712***	.0385*	-.0441
	(.109)	(.0147)	(.0252)	(.0241)	(.0220)	(.0308)
Borough and Quarter Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Demographic Controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations (borough-quarter-year)	1428	1428	1428	1428	1428	1428

Notes: *** denotes significance at 1%, ** at 5%, and * at 10% level. The dependent variable is the number of Class-A drug related hospital admissions per 1000 of the population in the cohort where the primary diagnosis refers to a Class-A drug. Class-A drugs include cocaine, opioids, and hallucinogens. Observations are at the quarter-borough-year level and are weighted by population of the borough relative to the population of London. Standard errors are robust to the presence of heteroskedasticity. The sample period runs from April 1997 to December 2009. The Policy-Period dummy variable is equal to one from Q3 2001 to Q2 2002, and zero otherwise. The Post-Policy dummy is equal to one from Q3 2002 onwards, and zero otherwise. All specifications include borough and quarter fixed effects, and control for shares of the population aged under 5 and over 75 at the borough-year level, and borough-quarter-year level admissions for malignant neoplasm, diseases of the eye and ear, diseases of the circulatory system, diseases of the respiratory system, and diseases of the digestive system. All these admission rates are also derived from the HES administrative records at the borough-quarter-year level. The estimates on the interaction terms, Post-Policy x Lambeth coefficients are produced by taking the double difference of the conditional expected values for the four Lambeth (0 and 1) x Post Policy (0 and 1) cells.

Figure 1A: Impact of the LCWS by Period Post-Policy Among Those Without Previous Admissions for Drugs/Alcohol
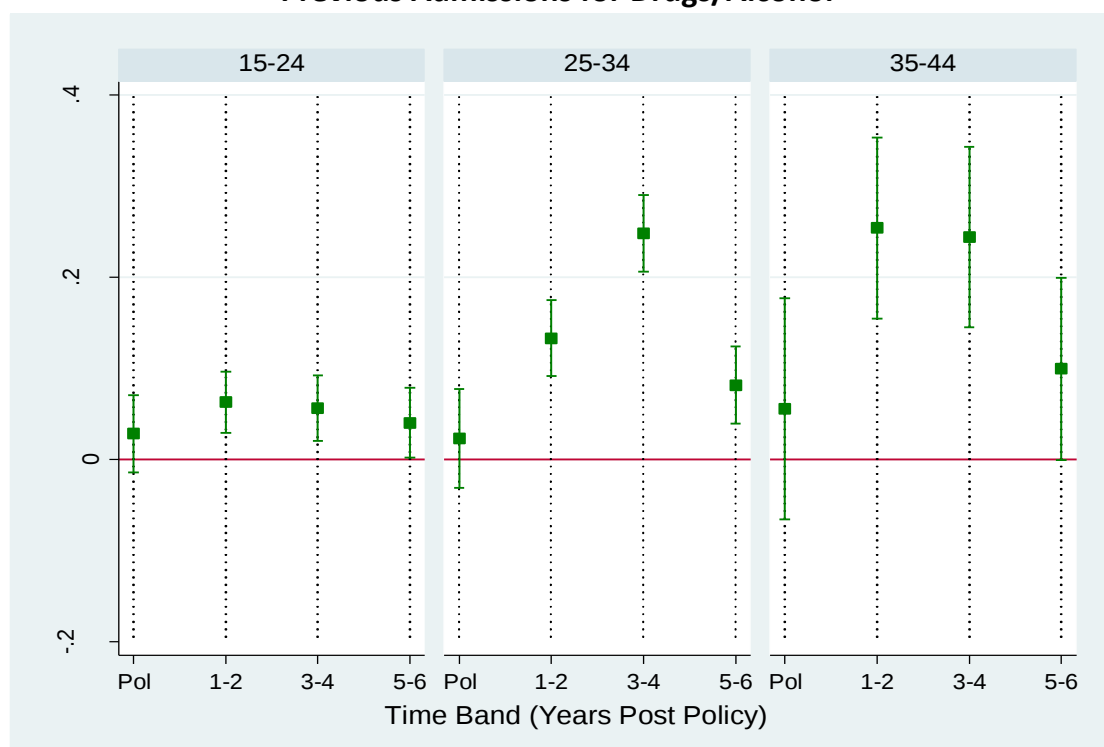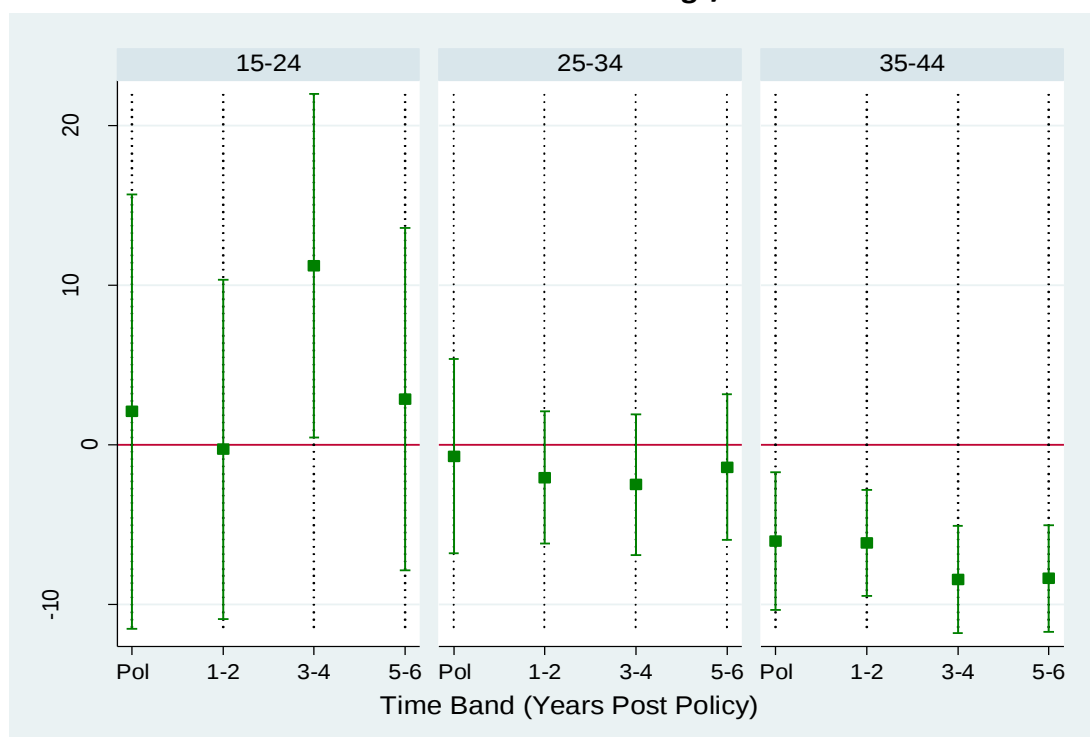
Figure 1B: Impact of the LCWS by Period Post-Policy Among Those with Previous Admissions for Drugs/Alcohol

Notes: Each panel in Figures 1A and 1B refers to a separate specification, where the policy and post policy dummies are replaced by four time dummies: the policy period (Q3 2001-Q2 2002); 1-2 years post reform (Q3 2002 - Q2 2004); 3-4 years post reform (Q3 2004 - Q2 2006); and, 5-6 years post reform (Q3 2006 - Q2 2008). The pre-policy period is the omitted category. Data from Q3 2008 onwards is excluded. Each plotted square corresponds to the Lambeth x Time Band dummy coefficient. The vertical green lines give the 95% confidence intervals. Age refers to age on the eve of the LCWS introduction (1st July 2001). In Figure 1A the sample includes all those who were not admitted for drug or alcohol use in the pre-policy period. In Figure 1B the sample includes all those who were previously admitted for drug or alcohol use in the pre-policy period.